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ORIGINAL ARTICLES.

DIGITAL EXPLORATION OF THE BLADDER, WITH REPORT OF TEN CASES, INCLUDING TWO PROSTATOTOMIES.[1]

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The exploration of the bladder in the female by the finger, introduced through the dilated urethra is a recognized surgical procedure of long standing. By an incision from the perineum into the membranous urethra, a condition analogous anatomically to the natural structure of the female is artificially produced in the male; for the exploring finger can then be introduced at a point corresponding to the location of the urinary meatus of the female; that portion of the urethra which is solely a genital organ and peculiarly masculine—the penile portion—being thus avoided, the bladder of the male is thereby rendered quite as accessible as is that of the female. This method of exploring the bladder in the male by means of perineal urethrotomy—brought prominently before the profession as a systematic means of diagnosis some three years ago by Sir Henry Thompson and Mr. Whitehead—often affords the only means for the recognition and relief of obscure vesical disease. By it the surgeon is also enabled to differentiate between disorders of the kidney and of the bladder when all other diagnostic means, even the microscope, have failed; and he can often readily relieve a painful condition that has long been blindly and unsuccessfully treated under the comfortable and comprehensive designation of "cystitis." This term cystitis, by the way, should be relegated—with jaundice and dropsy—from the list of diseases to the catalogue of symptoms; since it is apparently always secondary to some tangible abnormality in the urinary or pelvic organs or in the spinal cord. Yet simple, safe, and valuable as the operation is, it can be made unnecessarily, and even with disastrous results. Like many other surgical proceedings, it is safe only when the surgeon possesses not merely the requisite manual dexterity but also an intelligent comprehension of the conditions involved. It is the object of this paper to offer some suggestions, based on experience and observation, as to the indications for *not* performing, as well as for employing, this operation.

[1] Read in the Section on Surgery, at the Thirty-Seventh Annual Meeting of the American Medical Association.

During the past eighteen months it has been my lot to make a digital exploration of the bladder, by perineal urethrotomy, in nineteen adult males. In seven cases the exploration was incidental to the performance of membranous urethrotomy for stricture and afforded no particular information; in the remaining twelve cases the operation was made as a means either of diagnosis or of treatment, or of both, as shown in the following brief résumé:

Case 1.—George G., 58 years old, came under my care April 29, 1885. Has had gradually increasing difficulty in urination for four years. Two years ago he had complete retention; was relieved by catheter. On that occasion the urine drawn was he says "like glue." Some bleeding followed the withdrawal of the catheter. Since that time urination has been frequent, painful and straining; he often makes several unsuccessful efforts before the urine appears; is frequently compelled to use a catheter; urinates about every hour day and night. Defecation occasions severe pain in the perineum and penis. Has been treated for cystitis and for prostatic enlargement without appreciable improvement. On examination the prostate was found symmetrically and moderately enlarged; catheter shows 6 to 8 ounces of residual urine; the eye of the catheter is filled with clotted blood; a drachm or two of blood follows the withdrawal of the instrument. The urine becomes semi-gelatinous on standing a few minutes. With the microscope there are found some pus and many blood corpuscles. A diagnosis of villous growth from the prostate is made with entire confidence.

Operation May 1: The finger detected just to the left of the urethral orifice, springing from the prostate, a villous growth with a circular base about half an inch in diameter; and imbedded in its meshes two calculi as large as a pea and a bean respectively. Tumor and stones were removed and the site of the growth thoroughly scraped with the curette. Rather free hæmorrhage was controlled by hot water and pressure. Convalescence was uninterrupted; the perineal wound was completely healed on the 17th day. Patient resumed work. Urination was free and painless; he rose usually once during the night to evacuate the bladder. June 22 he observed for the first time a little blood at the end of urination; this was repeated several times up to the end of July, at which time I lost sight of him.

The interesting feature of this case was the diagnosis; the villous nature of the growth was predicted upon the tendency to hæmorrhage, the rapid clotting

of the blood, but especially upon the gelatinous appearance of the urine after standing a short time; that it grew from the prostate was considered almost certain in view of the pain along the urethra and in the perineum.

Case 2.—C. G., 17 years old, was referred to me in October, 1884. For two years, he had had gradually increasing frequency and pain in urinating; for one year the urine had contained considerable pus and albumen. On examination I found no objective symptoms in the urinary or genital organs; subjectively there was pain and tenderness of the urethra and bladder neck; urination every half-hour day and night.

Diagnosis not certain, but *tuberculosis of the kidney pelvis* is suspected, though no bacilli were found in the pus examined at that time.

At my advice, he went in November as a forlorn hope, to Southern California. For three weeks after arrival he improved rapidly in both and general symptoms; then after exposure in the rain, a decided aggravation of the disorder ensued. The local physician assured the patient and his mother that there was no tuberculosis about the case and that complete and permanent relief would be obtained by opening the bladder. In April he returned to Chicago and I was requested to perform the operation, but declined on the ground that it would be useless; for I was now enabled to confirm the diagnosis of tuberculosis by the discovery of the characteristic bacilli in the pus expelled with the urine. Finally, however, when it became apparent that the patient and his friends were determined to have the operation performed by some one, I consented. May 18 I explored the bladder with absolutely negative results. A slight improvement in the local symptoms lasted for some weeks.

Examination November 21 showed nodular enlargement of the right epididymis, typical of tubercular disease. The patient's local symptoms were about the same as before, his general health somewhat worse. In this case, the important feature was the diagnosis, which was necessarily somewhat doubtful until the bacilli were discovered in the urinary pus; and even then the possibility that an additional undiscovered factor might be present—a vesical calculus or growth—was a reason for operation. The case illustrates the established but often neglected fact, that extreme irritability of the bladder and tenderness of the urethra can be caused by disease of the kidney-pelvis.[1] An unpleasant result was a persistent, though very slight perineal fistula, caused by my failure to remove the drainage tube until nearly three weeks after operation.

Case 3.—Michael H., 63 years old. Gives a history of cystitis for four or five years. Complete retention two years ago. Admitted to County Hospital, April 14, 1885, for retention; relieved by catheter. Was sounded for stone with negative result; the case was regarded as one of enlarged prostate, and the bladder was irrigated daily. May 1 was transferred to my service; found him very weak, emaciated and feverish, temperature ranging from 101 to 104, pulse from 110 to 140; has had irregular chills for two weeks past. On examination per rectum I could detect no prostatic enlargement, but felt a sac about as thick as a lead-pencil containing numerous small calculi, and extending from the left apex of the prostate upward along the rectum as far as the finger could reach. My diagnosis was concretions in the seminal vesicle and consequent cystitis. As the patient's condition was critical I determined upon immediate operation.

May 1, with the assistance of Drs. Miller and Fenger, I explored the bladder. Upon inserting the finger into the bladder I found that the concretions were more plainly felt there than by the finger in the rectum. Hence we concluded that they must be contained not in the seminal vesicle, but in a diverticulum of the bladder. Yet it was impossible to secure a direct contact with the stones, as they were everywhere covered with mucous membrane. A little search revealed at the lower extremity of the mass, a slight depression in the surface of the bladder. The mucous membrane at this point was torn by the finger-nail, which thereupon came into contact with a calculus. The opening was enlarged and calculi ranging in size from a pea to a hazel-nut were scooped out of the cavity, one after another, until seventeen lay upon the table. The patient rallied nicely from the operation; his pulse and temperature became nearly normal, and for two weeks I entertained, in spite of his previous condition, a hope that he might recover. At the end of the third week, however, he began to exhibit an evening rise of temperature, and he finally died June 12, six weeks after the operation.

Case 4.—John A., 38 years old. Was admitted to the County Hospital October 16, 1885. For over a year he has been compelled to urinate frequently and has had pain above the pubes and along the urethra. These symptoms gradually increased and were repeatedly complicated with retention of urine; during the last six months he has constantly carried, and frequently been compelled to use, a catheter. On admission he is attempting to urinate, with but little success, every fifteen minutes day and night; and complains of constant and severe pain, aggravated during urination, along the urethra, in the glans and behind the symphysis. The urethra is so tender that he yells with pain upon any attempt to introduce a rigid instrument. With a soft catheter it is ascertained that there is no decided stricture and that the entrance to the bladder is unobstructed. There is no prostatic enlargement; the urine contains a little pus.

Diagnosis: A tumor, nature unknown, though probably neither malignant nor villous, growing from the prostate.

October 23 I explored the bladder. The finger on entering the bladder pushed before it a smooth body which was found to be of doughy consistence, globular form, about as large as a walnut, and attached by a rather narrow pedicle to the upper margin of the urethral orifice.

The symptoms were thus clearly explained; for

[1] This patient, who passed out of my hands a year ago, died August 16, 1886. At the autopsy there was found some tuberculosis of both lungs and advanced tuberculosis of the right kidney, but little of the renal tissue remaining; the left kidney was enlarged, fatty, but not tuberculous; prostate and bladder apparently free from tubercles.

Until shortly before death there were no symptoms, subjective or objective, to indicate which kidney was the seat of the disease.

the tumor—suspended against the urethral orifice by its narrow pedicle—evidently acted like a valve; for while it was readily pushed aside by the catheter from without, it effectually prevented the escape of urine from within the bladder, and thus produced the symptoms of a tight stricture.

Query—Are there not possibly cases of similar nature circulating around among surgeons and labeled "spasmodic stricture"?

Presuming that it was a fibrous or mucous polyp, I inserted through the wound a small stone forceps with the intention of twisting off the pedicle. While I was introducing my finger alongside of the forceps in order to control its extremity, about half an ounce of watery fluid (which I then supposed was urine) escaped from the bladder. I then discovered that the tumor had disappeared; but by a careful search I found a very delicate membrane, evidently a collapsed cyst, resting against the bladder wall. As I could not seize it with any instrument at my disposal, I tore the sac as completely as possible with the finger nail.

For two days subsequently the patient suffered excruciating pain along the urethra; but his convalescence was unusually rapid, the wound being entirely healed on the 9th day. At first he was compelled to urinate every two or three hours, day and night; but this discomfort—apparently a mere habit of the bladder—gradually disappeared. For four months past his urinary functions have been perfectly painless and normal.

Case 5.—M. W., 38 years old, was admitted to the County Hospital November 3. For several months he had experienced pain and difficulty in urinating, alternating with incontinence. His urine contained much pus; his bladder was sometimes distended, and at other times empty; there was no stricture nor prostatic enlargement. For several weeks before admission he had suffered from irregular chills followed by fever and sweating; his general health was much impaired; his condition in short was that described as "typhoid."

It was evident that he was suffering from cystitis and pyelitis, though I was unable to recognize the cause; nevertheless I considered perineal urethrotomy justifiable as a means of relief from this condition.

November 14 the bladder was explored and drained; nothing abnormal was found. Yet the expected results were attained; the chills and fever ceased, pulse and temperature became normal, the appetite returned, the urine became clear, in short the cystopyelitis subsided. After his convalescence from the operation when the patient again began to walk, I discovered the cause of his cystitis—namely an incipient transverse myelitis. Had he not been confined to his bed upon admission to the hospital, I would probably have detected this lesion; yet I would just as certainly have performed the operation, since without it he would soon have died of suppurative nephritis, the danger of which was now removed.

Cases 6, 7 and *8* were unfortunately instances of malignant tumor of the prostate and bladder. Two were men aged 69 and 58 years, respectively; in each of these the diagnosis—which was rendered almost certain by the history and physical examination—was made absolute by the discovery of numerous atypical epithelial cells in the urinary sediment under the microscope. The older of these two men was already bedridden from exhaustion when I first saw him; in this case I made perineal urethrotomy, and inserted a drainage-tube merely to obviate the agony of frequent urination. Such great relief was secured that the drainage was maintained until his death, three months later.

In the second case the patient was seen at an early stage of the malignant disease. Having located the growth in the left lobe of the prostate, I explored the bladder to ascertain the feasibility of an attempt at removal, intending, if excision of the neoplasm with the cautery seemed practicable, to make also a supra-pubic cystotomy for additional facility of manipulation. I found, however, that the growth not only involved the left lobe of the prostate, but was also prolonged, as a small, firm ridge, along the posterior wall to the vertex of the bladder. I therefore made no attempt at removal. The wound healed readily; the patient is still—four months later—able to walk, though steadily sinking under the malignant disease.

The third case of prostatic cancer presented an instructive feature. It was that of a man 31 years old, admitted to hospital with a violent and fetid cystitis and almost complete retention of urine. Per rectum the prostate was found greatly and irregularly enlarged, hard and knotty, making the diagnosis easy. Although large instruments readily entered the urethra as far as the prostate, I was unable to pass even the smallest catheter over the prostatic obstruction. I performed membranous urethrotomy and inserted a drainage-tube into the bladder.

Almost complete suppression of urine ensued; and in spite of fomentations to the loins and pilocarpine internally, the patient died of uræmia on the fourth day. At the autopsy there was found an epithelioma involving the prostate and floor of the bladder; the vesical walls were enormously hypertrophied, the kidney calyces were greatly dilated, and there was a pipe-stem calculus in the left pelvis. This case exemplifies the danger—not yet sufficiently recognized—of emptying the bladder in cases where, from long obstruction to the exit of urine, the bladder walls have become hypertrophied and the kidney pelves distended.

Case 9.—X. C., 59 years old, was admitted to hospital February 3, 1885, in the last stages of chronic cystitis, from which he had suffered for fifteen years. He had repeatedly had complete retention of urine; for five years has been obliged to urinate every hour or oftener, day and night, and for more than a year past has been practically unable to void urine except through a catheter. On admission he was in a typhoid state and suffering from irregular chills, fever and sweats. After vainly trying for several days to improve his condition by washing out the bladder, I made perineal urethrotomy February 9, and drained in the usual way. Immediate and complete relief was secured; on the second day his temperature became and remained normal.

Finding the cause of the cystitis in an immense

myoma—or so-called hypertrophy—of the prostate, I seized the opportunity to execute a plan which I had long cherished, namely, to make an artificial channel through the obstructing portion of the prostate.

Accordingly, two weeks later I reopened the perineal wound and with the galvano-cautery made a channel through the prostate sufficiently capacious to admit easily an ordinary lead pencil. To my surprise and gratification the patient showed absolutely no reaction, his temperature never exceeding 99° F. After the wound healed he easily retained urine three to four hours, and passed water without pain and without a catheter; the residual urine was reduced from eight ounces to half an ounce, showing that the bladder almost completely emptied itself; the urine was quite clear, even after standing. In short, he left the hospital April 3 a new man.

In consequence of the habitual retention of a little urine, I directed him to use the catheter once daily, an injunction which, after returning to his home, he usually ignored. As a consequence he had on two occasions during the following summer slight indications of cystitis, which, however, quickly subsided when the catheter was used once or twice daily.

September 12 he suddenly manifested signs of acute uræmia, of which he died, nearly seven months after the operation.

The uræmia was explained at the autopsy by the discovery of very small, contracted kidneys. I also secured the bladder and prostate, which are here submitted for your inspection.

Case 10 was a man 68 years old, with the usual history of cystitis from prostatic enlargement, extending over seven years. Although he was in a very exhausted condition, I attempted to repeat the operation of channeling the prostatic obstruction practiced successfully on the former patient (Case 9). In the midst of the operation the battery failed, so that my design could not be executed. The patient's ability to urinate and his comfort were decidedly increased by the incomplete operation; but his health did not improve, and he died some two months later.

The value and safety of digital exploration of the bladder through the membranous urethra as a means of diagnosis and treatment, are so clearly exemplified in these cases that I shall expend no time in expatiating upon them. I would merely remind you that the cases are in no wise selected ones, but are recorded as they successively occurred; that the majority of the cases belonged to that class of ill-conditioned individuals who drift into the wards of a large charity hospital; and that five of the ten were brought into the hospital in an apparently hopeless condition, to die.

Of the ten, three died within two months after operation; in one of these—the case of prostatic cancer, where death ensued in four days, from uræmia—the fatal result was unquestionably hastened and directly caused by the operation, though without operation this man could not have lived many days. In the two other fatal cases—where death occurred some two months later—it is just as certain that the operation prolonged life; and had it been performed a year or two earlier, before the patients had become so exhausted, it would have been extremely beneficial.

As essential preliminaries to a diagnostic exploration of the bladder, I would adduce two considerations:

1. It should not be performed as a diagnostic measure until it is reasonably certain that the seat of the difficulty is in the bladder, or until all other and less radical diagnostic means, including the microscope, have been exhausted; otherwise it may yield only disappointment and regret to both surgeon and patient. For it must be remembered that an irritable condition of the bladder can be produced by causes without as well as within this viscus, such as stricture of the anterior urethra, tuberculosis or calculi of the kidney-pelvis, etc. In such cases the morbid symptoms may be exhibited chiefly or exclusively by the bladder, while the lesion lies chiefly or exclusively elsewhere. As examples I would mention my own case of tuberculosis, and the two cases of exploration of the bladder which I have seen performed by other surgeons; in both of these the operation was ill-considered and fruitless, since in each the lesion evidently lay in the renal pelvis.

2. The operation should never be performed until the bladder has been accustomed to complete evacuation, by catheter or otherwise. If in consequence of a tight stricture or prostatic obstruction, the bladder is much hypertrophied, membranous urethrotomy is dangerous, not from a probability of urinary extravasation, but simply because a cysto-pyelitis of more or less gravity—possibly even anuria—usually follows the sudden removal of the accustomed high pressure in the bladder and kidney pelvis. Examples are found not only in my own fatal case of anuria, but also unfortunately in the daily introduction of catheters for retention, division of tight strictures, lithotomies, etc. Therefore, before the bladder is explored in a case with tight stricture, prostatic enlargement or other cause of vesical hypertrophy, the bladder should be gradually accustomed, in the course of weeks, to complete evacuation; the stricture should be enlarged; or the prostatic obstruction should be overcome by the habitual use of the catheter until all residual urine is withdrawn. Then, and *not till* then, can the surgeon rest assured that digital exploration of the bladder from the membranous urethra, whether it afford much or little benefit, will at least be safe and devoid of injury.

ON THE RADICAL CURE OF INGUINAL HERNIA. A REVIEW OF THE EXISTING STATUS OF THE OPERATION, WITH REMARKS ON ITS PAST HISTORY.[1]

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Few subjects are more attractive to the surgeon than that of hernia, whether viewed from the standpoint of pathology or that of surgical therapeutics. The ablest surgeons in past ages, as I shall presently

[1] Read before the Chicago Medical Society, August 16, 1886.

show, devoted much of their attention to the subject, and although time has passed, and tome and thesis become dusty with age, and in many cases the theories themselves misty with the changes of time, and in others laid forever at rest by the solid anatomical facts hurled against them, there has been a golden thread of truth extending from the earliest time to our day. But this thread, however bright and shining, is only a thread; and while some things are proved concerning the guiding principles of the operation, yet the disease continues to afflict mankind, and until the ushering in of antiseptic cleanliness in all operations, the weight of surgical opinion in our day was against the attempt to cure hernia. Several such periods have come and gone in the history of this operation, and while nothing in history of the past may seem to warrant the disturbance of the present surgical lethargy, yet it *is* worth while to get our bearings, as the mariner takes a reckoning to find out where he is and to determine his future course.

Dr. Morell Mackenzie must have been secretly thinking of hernia when he commenced writing that able article in a recent number of the *Fortnightly Review*, entitled "Is Medicine a Progressive Science?" Let us then get our bearings; take from unused shelves the musty records of former surgeons, and having found where we are, let us see if some headway may not be made to remove the reproach. That the disease is sufficiently prevalent to be of interest Heaven knows. Dr. Baxter's tables show that out of 334,321 recruits and substitutes examined by the recruiting officers during the war of the rebellion, more than 17,000 were rejected on account of hernia. George Hamilton, of Liverpool, estimated that in England alone there were one and a quarter millions of persons suffering from hernia, and 88 per cent. of these hernias are inguinal. The London Truss Society, during the first twenty-eight years of its existence, had issued over 83,000 trusses to the ruptured in the city of London alone, and from information received by me since this paper was commenced, I find that two factories in the city of Philadelphia manufacture and sell from 216,000 to 250,000 trusses per annum! Nor do all the ruptured wear even this badge of incurability. Think of all the old incarcerated hernias and concealed hernias, where trusses cannot be worn, and where from fear of pain or from penury the patient will not or cannot buy a truss! Let surgery, then, apply itself anew to the task before it; for there is yet a world to conquer. Let it not be dismayed by the fatalistic evolutionists who hold that the weakness of the abdominal walls is a certain evidence that man has been evolved from beasts which did not walk erect, and that in the fulness of time when man has been long accustomed to walk upright, the abdominal walls may become tougher, or enveloped in a cartilaginous or bony case. It may be considered a little impatient, but I for my part have no desire to wait. I wish to stimulate experiment and inquiry, and when the dawn of that time shall come for which evolutionists sigh, when man shall be physically perfect, and it shall be said of this beautiful city, as of Troy—Chicago was—let it also be said of this disease: there was once a question as to how to cure a very painful and fatal disease, now happily obsolete, but the surgeons of that day settled it. This much is introductory; let us now briefly look up the history of the past, which, thanks to the labors of Sprengel and others by whom it has been compactly and succinctly stated, the task is an easy one.

HISTORY.

Celsus was the first surgeon to fully define the tunics of the scrotum and testicle, and he had definite ideas about the operation for the cure of hernia. He used cauterization and a bandage, probably a spica. Sprengel thinks Celsus borrowed his notions from Alexandria, for Leonidas of Alexandria had exploded the doctrine of the rupture of the peritoneum, and taught the practice of cauterization; he also in some cases scarified the parts and used astringents.

Paulus, who followed Celsus in almost paraphrastic terms, did not mention him, and the able translator of Paulus (Francis Adams), remarks, "But when did a Greek writer ever acknowledge himself under obligations to a Roman?" Thus, if you will pardon the digression, you will see that the Eastern custom of ignoring the progressive West, of which custom some of you may have heard of late, is of rather ancient date.

The Arabians, Hali Abbas and Ali, the son of Abbas, used astringents as topical applications, and in severe cases they practiced the actual cautery.

Ligature of the sac had been practiced from an early day, and Paulus and his Arabian followers described the operation. The red hot iron for many centuries was the favorite remedy.

Maupas was the first who recorded himself in favor of making a straight incision directly through the rings, performing a gastrorraphy, and stitching up the wound.

Lafranc, who flourished in 1296, favored castration according to the ancient methods, and the fastening of the stump as a plug in the wound. He, however, had three methods, one the thrusting of red-hot needle points into the tissues about the hernia, and he invented the first compression forceps, or clamp. Guy de Chauliac, while criticizing the methods of his predecessors, and also that of a certain Bernard Metis who, before completing his incision, tied the cord with a gold thread, the modern point Dore, still believed in the caustic, and occasionally the arsenical paste. He recommended rest in bed and strong compression. It was desirable, in the opinion of surgeons of that period, to provoke suppuration at the ring.

The method of operating for the cure of hernia being nearly always accompanied with ablation of the testicle, now fell into disuse, and quacks alone practiced any of these operations. A traveling Spanish hernia-cutter was brought to the notice of Alexandre Benedictus, who performed some happy cures by the use of a silk seton, fastened to an ivory button. The same inhabitants of higher Italy who were celebrated in the Middle Ages for stone-cutting, were also very successful as traveling herniotomists, if contemporaneous accounts may be believed, and the

methods were kept a profound secret; but as late as 1633 Jean Baptiste Cortesa relates that he saw at Messina a certain Ulysse de Norcia apply a caustic substance, the parts consumed by it, and a very deep scar result. Joseph Covillard saw a "vagabond" cure a hernia by caustics and the ligature. (Sprengel).

Some of these peripatetic herniaotomists, after opening the sac, replaced the testicle in the abdomen, and drew the ring together with a gold thread. Scultetus relates that in his time the anabaptists frequently performed this operation in a similar manner. Fallopius practiced the ligature and castration. He did not object to the methods of his predecessors, but limited and modified them. He restored the gut after incision, by taxis and the use of oil of rose with white of egg; he used gold thread when he ligated the cord, and he scarified the rings. Ambroise Pare was the first to absolutely abandon castration. He used astringents, a bandage with iron filings to the surface as a counter-irritant, and powdered iron in the wound. He performed gastrorraphy in aggravated cases, and introduced a gold wire to separate the cord from the intestines.

About this time the treatise of Pierre Franco appeared, which in the light of modern anatomy once more dissipated the ancient error that hernia was always accompanied with a rupture of the peritoneum.

Freytag, a Zurich surgeon, was the first to practice dilatation of the rings in strangulated hernia, having first made incision. He then attached the peritoneum to the ring with numerous sutures.

Abraham Cypriaan, of Amsterdam, systematically dilated the rings with the finger until reduction was effected in the case of strangulated hernia, then with a two-edged knife he separated all the tissues about the ring and canal.

Nicholas le Quin, of Paris, and his nephew Antoine le Quin, appear to have introduced the truss about the year 1680, although the spica bandage had been used from the earliest times. In the time of Louis XIV operative procedures by the faculty had once more fallen into disuse, and the quacks reaped a harvest. A prior of Languedoc, named Cabrieres, communicated to the monarch under promise of secresy during his life time, an infallible mixture which was never-failing in its power. This secret, when brought to light, was found to consist in the administration of muriatic acid diluted with red wine, and the external application of a pitch cerate under pressure.

Dionis wrote a thesis on this subject in which he recommended the opening of the sac with caution; he taught the principle of dilatation of the ring, and he tied the omentum. After the operation he introduced into the ring a large dossil soaked in yolk of egg and oil, kept it in place by a compress of charpie adjusted by a spica bandage.

Vauguyon was a close follower of Dionis, but he described the technique of the operation in more detail. He used the grooved director, not only for raising the tissues for division, but also for dilating the ring. He, after the restoration of the intestine, scarified the ring, and applied a bandage like Dionis. The long controversy as to whether the peritoneum was really ruptured or not, was not settled until the beginning of the eighteenth century.

Michel Louis Renelaulme de la Garanne, while insisting there was a rupture of the peritoneum, operated in all cases except those of incarceration, by the operation as performed by Dionis; he used in addition to the ordinary bandage, a suspensory bandage. This was, many years later, brought forward by Mayor as a new procedure.

While French surgeons had thus come to follow a definite line of practice in these cases, the rest of the world, so far as known, still followed the Celsian plan of castration with cauterization.

Françoise Wiedeman, in 1719, was the first German to defend the new treatment of hernia adopted in France at this period, Jean Henry Freytag, of Switzerland, in 1721 rejected the castration and as well the point Dore. He still favored the scarification as practiced by his father, Jean Conrad Freytag, and recommended the excision of the projecting portion of the omentum, when necessary.

Mauchard in operating recommended that the bistoury be held flatwise (after the sac had been opened carefully), and he did not raise the membrane with the cellular tissue, when he had pierced the peritoneum, he introduced a sound, lifted the membrane and examined the state of the intestines. He then scarified the ring, and tied the sac with a waxed thread.

Heister recommended a truss, which, upon reference to the plate, is seen to be an oval pad attached to a strap, evidently intended to buckle around the waist.

He approves of Petit's operation; by this operation Heister says Petit cured a "tailor who was well within five days of the operation." Heister, however, advises that the bandage alone be trusted in large hernias. Incarcerated hernias he endeavored to reduce by the taxis, and having done so, he applied the "spica bandage, which should not be left off by the patient for many years, and if he be old, it should be worn during life. I have sometimes known a clyster of the smoke of tobacco succeed in relaxing the parts, when others have failed." "A large clysma fumosum," he says, "of the common English or weak tobacco was injected into a poor patient under this disorder, but with no effect, but the smoke of strong Virginia tobacco quickly gave the patient a stool, and the prolapsed intestines soon returned into the abdomen of themselves." Heister, it is to be observed, when he did operate, anticipated certain so-called antiseptic precautions, by the cleanliness he required, and the shaving of the groin as preliminary to the operation. He always opened the sac and returned the hernia, without division if he could; if practicable, he divided the ring with care and applied the pledget or dossil of lint and a spica bandage; but then, as now, there were many attempts to escape the knife, the bandage, and the truss, and John Douglass states, in his "Syllabus of Chirurgical Operations," that a man named Little-John made great reputation throughout England by a secret remedy for which he was paid five thousand pounds by King George the First, who also added to this sum an annual pension of five hundred pounds.

When this remedy was promulgated, it was found to be cautery with sulphuric acid; and when the granulations sprung up they were touched with the *lapis infernalis*. A bandage to retain a plaster on the cicatrix completed this rather too highly appreciated discovery. If Little-John had lived in this day he might easily have increased his emoluments by starting a "sanitarium." We recognize this cauterization plan as an old acquaintance. And so the changes have rung, the same old story with but few variations from Celsus down. I will not weary you with continuing in this history an account of the methods of Alexander Monroe, Jean Georges Wagner, of Lubek, Lafay, Samuel Sharp, and Tacconi, for they were all simply modifications of the preceding plans. It was not until near the middle of the eighteenth century that surgeons began to cut off the gangrenous portions of intestines in cases of strangulated hernia. Claud Amyand, Montabourg, Peyronie, Ramdohr, Mery, were among the first to practice and recommend this procedure.

The glance we have thus given at what may now be termed ancient surgery of hernia, shows clearly enough how little may be hoped for as new, and yet how infinite the variations of the few fixed principles. If I do not quote, among the moderns, Percival Pott, Sir Astley Cooper, Benjamin Bell, Mohrenheine, Barthol and Saviard, Robertson, Dessault and Cline, it is simply because these excellent surgeons, some of of them masters, simply threshed the old straw. The Spanish surgeon Antoine de Gimbernat, in his "new method" of operating in crural hernia, Madrid, 1793, did develop some originality in description of the anatomy of crural hernia, but the field seemed to be worked out. Little change in opinion or sentiment took place until about 1840, when the method of creating inflammation of the whole canal by injection was again brought into notice, but it will now be more convenient and easy to follow if the different operations are adverted to seriatim.

Castration, first proposed by Celsus, and, as seen in the foregoing section, followed by surgeons for several centuries, has been long abandoned, although in cases where the hernia co-exists with disease of the testicle, as in the case reported by Nott, of Mobile, in 1847 (in which the lead wire was used), castration may very properly be an accompaniment of the operation.

Cauterization was formerly used by the Moxa to the surface, by red-hot irons, and by red-hot needles thrust into the tissues, and by mineral acids. Almost every known caustic has at some time or another been used for the cure of hernia. The galvano-cautery was proposed by Dr. John C. Minor, of New York (*Am. Jour. Electrology and Neurology*, 1879).

Topical Applications.—These have been the favorite remedies of charlatans in all ages, and have been occasionally used by the faculty, but being utterly unscientific in principle, are now abandoned.

Ligature of the Sac.—This operation is now practiced in connection with the so-called "antiseptic" operation, but in the modern operation the sac is opened, while in that of the ancients the sac was frequently ligated without opening. Ambroise Paré is erroneously given the credit of the priority in this procedure. Malgaigne speaks of various kinds of ligature, including catgut. The elastic ligature has been brought forward. Molliere, in "Memoires Sociéte des Lyons, 1877," proposed to occlude the neck of the sac by the elastic ligature. His conclusions were that the ligature of the sac was usually necessary and gradual ligature was the best method, and, in passing, it is proper to revert to the fact that Paulus Ægineta proposed and practiced sac ligation. Gallaud, in 1878, favored the elastic ligature.

The Royal Suture.—The sac being entirely exposed, it was raised and stitched, after which the portion outside the suture was cut away. The term "royal" is somewhat whimsically said to have proceeded from the fact that the King's subjects were saved thereby.

Scarifications.—This practice is a very ancient one, and has been occasionally practiced down to a very recent date, with excellent results. These scarifications were at first practiced on the sac alone, but finally on the pillars and the entire canal. Dr. G. N. Fitch, of Chicago, in 1845 scarified the neck of the sac and produced a cure (*Indiana and Illinois Medical Journal*, N. S., Vol. IV). Guerin was probably the first to practice subcutaneous scarification. The so-called Heatonian method has, as one of its advantages, that the tissues about the pillar and canal are punctured.

Invagination (a) *Without Suture.*—This practice has for its object the occlusion of the inguinal ring by the fascia and sometimes by the integuments. Leroy D'Etoilles proposed invagination without suture, the invagination to be retained by a plug of rubber and retained by a bandage. Aitchison, in the *Indian Lancet*, September 15. 1860, proposed to invaginate the scrotum by a rubber tube carried into the ring by a solid plug. The plug was then to be withdrawn and the tube filled with air and retained with a bandage. This method he claimed could be used with or without suture, but this procedure was in fact long antedated by the truss pad of Dr. P. H. Cabell, of Alabama, who had constructed a truss pad with what he called a "finger-like process." (*Virginia Medical and Surgical Journal*, vol. iv, 1855.)

Invagination (b) *with Suture* has been practised for a longer period, and has been very successful. It in principle forms the basis of many of the more famous operations, such as Gerdy's and Wutzer's. In Gerdy's operation the scrotum is invaginated on the finger and pushed into the ring. A double-threaded needle is thrust into the bottom of the cul-de-sac, brought out through the abdominal parietes, the needle withdrawn, reinserted at a little distance, withdrawn and the threads tied. This cul-de-sac, if left to itself, would prove a nuisance on account of the retention of moisture, and the skin was therefore denuded of its cuticle by repeated cauterizations with ammonia, and when granulations sprang up the surfaces were made to adhere by pressure. Bransby Cooper proposed to destroy the cuticle of the invaginated portion by actual cautery at a single operation. This method has fallen into disuse, although occasionally revived in our time. The late Dr. Geo. T. Allen, of Springfield, Ill., reported fifty cases cured by what

was practically Gerdy's method. (Trans. Illinois State Med. Soc., 1867–8.) Wutzer proposed to invaginate the scrotum by a wooden plug having a curved needle at the end. After the tissues were invaginated and the needle thrust through the tissues, a fenestrated metal plate was placed on the outside, with the needle projecting through the fenester, and clamped down tightly by means of a screw at the end of the plug. The apparatus is kept in place for about a week and a truss worn for some months. In Wutzer's first series of cases there were fifty-seven cases and no deaths. This operation was very popular in its day, but is now superseded by a method which might well be termed the combination method, by which I of course refer to Wood's method, which not only invaginates the fascia but draws together the pillars. While all these methods have their greatest success in small herniæ and bubonoceles, yet both Gerdy's and Wutzer's are absolutely valueless in those which are so large that an ordinary invagination will not occlude the opening. As Wood's operation is so recent and so well known, I will not in the presence of this audience stop to describe it. Mr. Wood's first case is recorded in the Transactions of the Medical Society of King's College, 1857–8.

Lawrence, who wrote the well known treatise on hernia, is frequently but erroneously given the priority of suggesting the drawing together of the inguinal rings by suture. The priority is probably due to those ancient surgeons who operated by incision through the canal. The varieties of suture used are almost endless, and it must provoke a smile to see in every country this operation when performed by any of these sutures described as new. In our country we point with pride to the fact that Physick recommended animal sutures. Levert, Nott and Gross used lead and silver wire, but it is known that Rhazes, the Arabian, described sutures made of harp or lute strings, with which he stitched wounds of the abdomen, and that there is no doubt that metallic suture wires of the less noble metals are really contemporaneous with the gold threads. To prevent the alleged untying of the catgut ligature, MacEwen, of Glasgow, in 1880 proposed to soak them in chromic acid.

Wood's operation has lately taken a new start by reason of the greater care in making incisions; insuring cleanliness; apposition of the parts, the use of germicide solutions and the excusion of air from the wound. Dr. H. O. Marcy, of Boston, on October 11, 1871, reported cases treated by this method. Prof. Fayrer, of Calcutta, in *Medical Times and Gazette*, April, 1872, reports fifty cases, of which forty-two were cured. Prof. Annandale and Sir William MacCormac give the priority to Dr. Chas. Steele, of Bristol, but Dr. Steele's first case was reported in the *British Medical Journal*, Nov. 7, 1874, more than two years after those of the Boston surgeon.

Marcy and Isidor Israelsohn are undoubtedly the pioneers in this application of antiseptic surgery.

Dr. Raye, of Calcutta, in the Trans. Med. Soc. of Calcutta, 1883, reports sixteen cases with twelve cured, one died, and two relieved, one under treatment on the date of the report. Swinton Edwards in 1884 reported three cases cured by this operation. He used drainage, iodoform, gauze packing, and an elastic bandage.

In a discussion in the Liverpool Medical Institution in 1883, opened by Mr. George Hamilton, Dr. Alexander stated that he had performed the radical cure thirty times without any death. Mr. Banks stated that he had operated on more than fifty cases, but preferred wire to catgut. Mr. Puzey thought that the patients should be kept in bed for a considerable time, as if the patients got up too soon the tumor reappeared. This caution, in my judgment, is a necessary one, for in one of my own cases operated upon in May, 1886, at the Providence Hospital, Washington, an inguinal hernia was converted into a bubonocele by the reopening of the internal ring. The patient in this case was allowed to get out of bed as soon as the wound was healed.

In a debate on the radical cure of hernia in the Academy of Medicine in Dublin, March 12, 1884, there was entire agreement both as to the propriety of the operation and of the excision of the sac. As to the general results of Wood's operation, Mr. Wood himself reports that out of 339 cases without special antiseptic precautions, and with wire sutures, ninety-six were cured, seven died, and fifty-nine failed; in the remainder the result could not be ascertained. The proportion of the unknown cases was presumably in about the same ratio. Nussbaum states that in half his cases the hernia returned, and in those reported by Leisrink one-third returned. There can be no doubt, then, that while much good has resulted from the operation, there is yet much needed to perfect its details. I wish it understood when I speak of Wood's operation, I mean as at present performed; that is, the "open" operation. Dr. Robert Abbe, of New York, recently reported twenty-one cases of this operation, with two deaths; in one case, however, the death was due to suppuration following an operation for varicose veins of the leg, which had been operated upon at the same time. His cases were too recent to give any account of the permanency of the cure.

One modification of Wood's operation has been proposed by Mr. W. Dunnett Spanton (*British Medical Journal*, Dec. 11, 1880), which consists in bringing the pillars together by means of a corkscrew wire mounted on a handle. A small puncture being made from above, the wire is placed in position, and forced down by a screw motion, the screw entering the pillars alternately. Mr. Spanton has operated on thirty-four cases with the screw and catgut suture with no deaths. This proceeding differs little in principle from that practised by Dr. Grenville Dowell, who used a needle with a deep curve, and brought the pillars together by suture. Dr. Dowell reported in his book published in Philadelphia, 1876, ninety-six operations by nine different operators with a result of eighty cures and sixteen failures.

Accidents may follow Wood's operations as well as the others, such as sloughing, peritonitis and tetanus. On this last point Surgeon-Major French reported (*Indian Annals of Med. Sci.*, 1876) that two of his six cases were followed by tetanus. He thinks it likely that the climate may have been at fault. From

the statistics which I have had compiled from official reports on file in the Marine Hospital Bureau at Washington, it appears that in Calcutta for the year 1884 there were 1,137 deaths from tetanus, out of a total number of deaths from all causes of 12,823—nearly 9 per cent.; so there really seems to be more than a conjecture back of Surgeon-Major French's suggestion. The record by months is as follows:

TETANUS IN CALCUTTA, INDIA, 1884.

	Deaths from tetanus.	Total deaths from all causes.
January	64	919
February	61	964
March	114	1,516
April	105	1,709
May	98	1,332
June	71	797
July	85	7·9
August	101	785
September	102	816
October	112	869
November	115	1,260
December	89	1,127
Total for year	1,137	12,823

Percentage of deaths from tetanus to total deaths nearly 9 per cent.

"In Sweden an 'improved' operation for the radical cure of hernia has for some time past been practised by Drs. Svensson and Erdmann, surgeons to the Sabbatsberg Hospital at Stockholm. A ligature is applied to the neck of the hernia, and the sac is cut off below the ligature, the contents being previously examined by means of an incision into the sac and returned; or if only omental, excised, together with the sac. In congenital herniæ the upper part of the sac only is removed, and where the large bowel is included in the hernia and adherent to the sac wall, this, after being separated from the surrounding tissue, is returned together with the large intestine, and the rents of Poupart's ligament united by sutures.

"The dressing employed is iodoform and boracic acid, the wounds being washed with sublimate solutions. Since this has been substituted for carbolic gauze, abscesses, which used to occur frequently, have become rare. Of the forty-eight cases thus operated on, none of which were selected, thirty-eight were permanently cured; at least, no return of the hernia occurred within six months; and in the cases where a return did take place, which amounted to 20 per cent., the condition was very much less painful and distressing than it had been previous to the operation.

"Sabbatsberg Hospital has now been opened six years and a half, and during that time 300 cases of hernia have been admitted, about 200 of these being operated on with the knife, a milder procedure, consisting of alcoholic injections, being employed in most of the earlier cases.

"Not a single case proved fatal, though some of the herniæ were very large, some reaching within three or four inches of the knee." (*Med. and Surg. Rep.*, Philadelphia, 1886, ix, 115.)

In regard to the necessity for the recumbent position in the after-treatment of these cases, it should be the rule, and neglect of this rule seems to be the cause of failure in many cases of recurrence. Ravin, indeed, many years ago proposed position as the only rational means of cure, applying the principle that any cavity in the body, when unused, became filled up, or its walls agglutinated by a mild inflammatory or adhesive process.

Two other methods of treatment, each popular enough in their day, I will not stop long to discuss. I refer to the injection, and the seton through the canal. Velpeau, in "Noveau elements de Medicine operative, par A. L. M. Velpeau, Paris, 1839," stated that in the application of the fact that iodine exerted a wonderfully curative influence on serous membranes in general, as instanced in hydrocele, he had used injections of iodine for the radical cure of hernia in 1835. He again used it at the Charity Hospital in 1840, and this method being a great advance upon those then in vogue, became the favorite one in Paris. It was used in America by Pancoast of Philadelphia in 1836, who claims priority; by Dr. William Jayne at the Penitentiary in Illinois in 1840, and a few years later by Heaton, of Boston, who changed the fluid finally to the fluid extract of quercus alba, but for many years kept his remedy a secret, for which he was tried by the American Medical Association. After Velpeau all Paris began to use iodine injections, and new trocars and canulas figure in the reports and theses of the period. Nélaton, Ricord, Jobert, Maissoneuve and Follin invented "new" trocars. Some opened the sac with a bistoury, and others, as Jobert, plunged the trocar directly into the sac. The statistics were, as usual, at first highly favorable, but finally deaths from peritonitis began to be frequently noticed, and the operation became somewhat restricted in its application. Heaton's injection being milder in its operation, and injected into the pillars and aponeurotic fibres instead of the sac, still survives, having been, as one may say, rejuvenated, by the twisted and altogether wonderful syringe of Warren.

Dr. Luton, of Reims (*Bull. Gén. de Thérap.*, 1877), proposed the injection of 10 to 15 drops of salt water.

The introduction of the seton is probably due to Rattier in 1835, but, as may be gathered, most claims of priority, in the present state of literature, are somewhat problematical. Dr. J. W. Riggs, of New York, in 1858, revived the seton and invented a needle for that purpose, as did Dr. Armsby, also of New York. Dr. J. M. Carnochan (in *Am. Med. Gazette*, vol. ix, 1858), reports cases cured by the "Riggs method," and Dr. R. Thompson reported a "new" instrument, carrying the seton through the inguinal canal, to the Ohio Medical Society in 1859, but the seton, for some reason or another, has gone to join the iodine injection, and the acupuncture practice of Bonnet (1837). Various other methods of plugging the ring are recorded, the autoplasty of Jameson, of Baltimore, Langenbeck and Graefe, and finally the testicle itself. Dr. Chas. T. Hunter recorded a case where by the patient constantly pressing the testicle up into the ring it became fastened there, and the patient was cured. Dr. Hunter thought this a unique case, but Michel (*Am. Jour. Med. Sci.*, Oct., 1878) speaks of this as an old Spanish method. It was not,

however, peculiar to Spain, although Henry Momiehm, writing in the early part of the seventeenth century, calls it a Spanish method, for both Garengeot and Scultetus in condemning it mention it as a recognized surgical procedure.

Let us pass the operations of Mayor by ligatures fastened on sponges, and his triangular "cravat" bandage, proposed in 1836; the punctures proposed by Anderson, of Baltimore, in 1835; the Cressom operation of 1838; the Belmas operation of gelatine strips in the canal; the modern revival of the suture royal by Czerney, to the last and most prevalent treatment, by continuous compression.

I have before spoken of the early invention of the truss, and you all know that there would not be space in this hall to exhibit the various modifications that have been invented. Hard pads, soft pads, air pads (Jeanney, 1838), springs and no springs, ratchets, catches and binding screws of infinite variety. Although radical cures of inguinal occur in mild cases, it is well known that trusses are in the main merely palliative; that permanent cures are rare, and closely confined to those of recent date with small openings, yet we go on ordering trusses for our patients just as if there were no way to increase the proportion of cures, and as if a truss could not as well be worn after an operation, if need be, as before it, and as if no recent advances in abdominal surgery had been made. As no logical reason can be given for a failure to accept the view that there has been an advance, I perhaps need not say that *I favor in all cases affording even a reasonable prospect of cure*, an operation therefor, and that *all cases whatsoever of bubonocele should be operated upon.*

Spontaneous cures of hernia, while uncommon, occasionally happen, and there are several cases on record. The most recent case is that by Dr. Streeter, of Glen's Falls, N. Y., where in consequence of suppuration in the inguinal canal following a bubo, the canal was closed, and the hernia cured. The patient had previously worn a truss for a year, without avail. But a surgeon would be hardly likely as a general practice to recommend his patients to acquire a bubo.

I conclude by quoting the remark of Sir Spencer Wells, made many years ago:

"But the surgeon who cures hernia radically with certainty and safety, is a greater public benefactor than he who saves the life of his patient in strangulated hernia, as he not only relieves a larger number of his fellow creatures from the suffering and inconvenience of wearing a truss, but he averts the danger of strangulation to which they are continually exposed in a greater or less degree through every period of life."

THE BEST FORM AND PRACTICAL VALUE OF THE SHADOW TEST IN THE MEASUREMENT OF REFRACTION.

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The purpose of my paper is to draw attention to the practical value of the shadow test, or retinoscopy; and to insist on the importance, for its successful and satisfactory employment, of certain conditions which have not received due attention at the hands of other writers upon the subject. In this country most ophthalmologists are not familiar with the test. See Dr. Loring's curt dismissal of it as of no practical importance; while of recent English writers Swanzy gives it eight pages of the seventy-three devoted to the whole subject of physiological optics and ametropia, Juler assigns to it ten, out of seventy given to errors of refraction, Hartridge one-eighth, and Morton one-sixth of the whole of their respective books on refraction.

I can readily understand how an acquaintance with it, based on some of the printed descriptions and a few unsuccessful attempts to apply it, would lead one to think lightly of its value. But no one who has mastered it, has felt, or, I think, will feel inclined to leave it unused; no matter what other methods he may have for the estimation of ametropia.

The principle of the test is simple. From each point of the illuminated retina rays of light pass through the dioptric media into the air. If by the proportions of these media, or a convex lens in front of them, these rays are rendered convergent, they meet in a real focus in front of the eye. If we determine the distance from this focus to the eye we determine the state of refraction in the eye, with or without a convex lens of known strength before it. At this focus the rays cross. Their relations become reversed. When an observer receives in his own eye these emergent retinal rays, if he receive them before they cross, he sees an erect image of the retina; if he receive them after they cross he sees an inverted image of the retina. Placing the eye alternately within and beyond the point at which they are focussed he sees first an erect, then an inverted image. The reversal of the image occurs at the conjugate focus of the retina. I have therefore called this *the point of reversal.*

As the point of reversal is approached from either side, the image of the retina becomes enormously magnified and very indistinct, and just at the point the pupil becomes occupied by a perfectly uniform feeble glare of light. Even some distance from this point the details of the fundus are quite difficult to see. An inspection of these details is not, therefore, a satisfactory method of determining the position of the point of reversal. The best object by which to determine if the retina is seen in the erect or an inverted image is a small area of light moved across it; obtained by throwing into the eye with a mirror the light from some lamp. By turning the mirror the light area is made to move across the retina in any desired direction, and the direction of its motion is readily known, for some of the light falls on the face, forming a light area there, and the areas on the retina and the face always move in the same direction. Now, watching through the pupil the apparent movement of the retinal light area, we can note when this is with the real movement, the retina being seen in the erect image, and when it is the opposite of the real movement, the retina being seen in the inverted image. And this direction of the movement can,

under proper conditions, be recognized close enough to the point of reversal to determine the position of that point with great accuracy.

Now there are two ways of fixing the position of the point of reversal. Either, as above indicated, the observer may move his eye first within, then beyond it, while the point itself remains fixed; or he may keep his eye fixed and move the point of reversal by placing in front of the patient's eye weaker and stronger glasses alternately until he knows just what glass will bring it to a certain position. The latter method has been followed by Cuignet[1] and the mass of those who have practised or written upon retinoscopy. The former was proposed by Chibret[2] for the determination of myopia; and by myself[3] for the determination of ametropia of all forms. It is the better way, because it is easier to vary the observer's distance than to make all the necessary changes of glasses, especially where, as in most cases, it is only desired to approximate the error of refraction to get a basis for subjective testing.

The variation of the distance between the observer and patient requires the use of the plane mirror. The plane mirror can be used for the other form, as described by Story.[4] But while the plane mirror answers for either, the concave mirror is only serviceable when the position of the observer is fixed, and that of the point of reversal varied. This is because the retinal light area must have a certain brilliancy and minuteness, for its motion to be readily watched. And with the concave mirror, if the observer approach the patient the retinal light area gets too large; if he withdraw from the patient it gets too dim. There is just one point at which the size and brilliancy of the light area are the same with the concave as with the plane mirror; its distance from the patient's eye is found by this proportion: $a—2d : 2a :: d : b$, in which a is the distance from the source of light to the mirror, d the focal length of the mirror, and b the desired distance from the mirror to the patient's face. Applying this formula, it is found that the greatest distance at which the concave mirror can advantageously be used without a specially brilliant light is but little over four times the focal distance of the mirror; if the light be placed back of the patient's head. For the ordinary concave mirror of 22 centimetres focal length, the proper distance is a little less than one metre. As I have pointed out in the paper above referred to, for accuracy it is best to make the final estimate of refraction with such a glass before the patient's eye as will bring the point of reversal over one and usually less than two metres from the eye. Morton and Barrett[5] have, apparently without knowledge of my paper, come to the same conclusion. But as they were working with the concave mirror, the common forms were found of too short focus. So they propose a special concave mirror with a focal length of thirty-six centimetres for accurate retinoscopy. Such a mirror could of course be used.

The need of a special form of mirror of course destroys the one advantage that retinoscopy with the concave mirror was supposed to have, namely: that we could practice it with the mirror of the ophthalmoscope already in our pockets. But for most American ophthalmologists that supposed advantage was most illusory. With us the segmental tilting mirror of Dr. Loring is commonly used, and for good reasons. But it is entirely unsuited to retinoscopy because it is too narrow. With it the light area is apt to pass entirely off the eye before the retinal area has moved sufficiently to indicate the direction of its apparent motion. I believe it is on account of the unsuitableness of the mirror used more than from anything else that the profession in this country has failed to avail itself of the shadow test. English and French ophthalmoscopes are almost invariably fitted with a round mirror over an inch in diameter; and even these are being discarded for larger mirrors specially adapted to the purpose. The form of mirror I use, and recommend, is that described in the current volume of the *Medical News*.[6] It is one and one-half inches in diameter, being quite as small as will, under all ordinary circumstances, prove satisfactory.

One other point as to the best method: the source of light should be small and brilliant. I have elsewhere given reasons for this; small, to get the characteristic appearances of astigmatism which become pronounced in so far as the retinal light area approaches the condition of a single bright point; brilliant, to make the margin of the area and its movements more readily discernable. The best source is unquestionably the arc electric light; but it is not generally available, and any bright lamp flame will answer. In my office I commonly use an argand gas burner, which is covered by a metal chimney with an aperture one inch in diameter opposite the brightest part of the flame. Placing the patient a couple of yards away from the light, I have a source of light small enough and bright enough to answer the purpose. As improving by contrast the brightness of the source of light, the room should be thoroughly darkened for the application of the shadow-test.

The practical value of a diagnostic procedure might be considered with reference to its capacity to reveal that which could be discovered in no other way, with reference to its certainty, its accuracy, and the readiness with which it is put in practice. For the mass of cases undoubtedly we have other ways of determining the refraction. But in a few cases retinoscopy is the only satisfactory and reliable way of doing this. For instance, where the patient cannot be relied on for subjective tests; and the eyes cannot be well fixed for the estimation of the refraction by the erect method, as when there is nystagmus. In several cases of nystagmus the shadow-test has shown me the presence and exact amount of considerable degrees of astigmatism, unsuspected after ex-

[1] Cuignet. Keratoscopie Rec. d. Ophthal., 1873.
[2] Chibret Detirmination Quantative, de la Myopie par la Keratoscopie Fantoscopie Retinienne a l'Aaide d'un Simple Miroir Plan. Ann. d'Oculist, 1882, p. 228.
[3] Jackson. Measurement of Refraction by the Shadow Test, or Retinoscopy. Am. Journal Med. Sciences, April, 1885.
[4] Story. Advantages of the Plane Ophthalmoscopic Mirror in Retinoscopy. Oph. Review, 1883, p. 228.
[5] Morton and Barrett. Clinical Investigations on Methods of Retinoscopy. Brit. Med. Journ., 1886, I., p. 105.
[6] Jackson. A Form of Plane Mirror for the Shadow Test. Medical News, 1886. I, p. 674.

aminations with the "refraction ophthalmoscope" and considerable testing with test-lenses. The case which first powerfully impressed me with the value of the test was one of a restless, wilful, spoiled girl of eleven. Where with the ophthalmoscope I could detect the presence of considerable hypermetropic astigmatism, but neither with this or the test-lenses could fix even approximately its amount or axis, her answers for subjective tests were utterly unsatisfactory and unreliable. Finally, in despair, I turned to the shadow-test which I had been studying and using in a rather tentative manner. With it I worked out the refraction and placed the lenses, R.—0.5 = +2. cy.ax.110°. L. +2.cy.ax.70°; bringing her vision for either eye from $\frac{1}{L^5}$, the best I had before been able to obtain, to $\frac{1}{X^5}$ partly. The surprise occasioned by the improvement in vision disarmed her of wilfulness, for the time, and I was able to get full subjective confirmation of the accuracy of the correction. Now, such is my confidence in the certainty of the method, that I would not hesitate in such a case to order the glass though the patient said it made the sight worse instead of better. For certainty an objective method, I think we all agree, is infinitely superior to any subjective test.

For accuracy, retinoscopy is considerably superior to the direct evamination with the ophthalmoscope. In hypermetropia, myopia and regular astigmatism, I think the limits of error may be placed as within a half dioptric. To discover irregular astigmatism and its character and extent, it is far superior to any other method of examination, as most of you doubtless know by experience; for it was applied for this purpose by Bowman and Donders long before Cuignet's description of "keratoscopy" was written. Loring says: "The determination of astigmatism by means of the ophthalmoscope has always been considered one of the most difficult and, from its want of accuracy, one of the least satisfactory applications of the instrument, and there is no doubt that this is to a great extent true." Here is the special field of retinoscopy. While Loring says of his method: "0.75D can be detected by this test, provided the accommodation in both the observed and observing eye is perfectly relaxed," it may be said of retinoscopy that under similar conditions of the observed eye, it will measure 0.25D of astigmatism, while the observer's eye may be entirely unskilled in the art of relaxing the accommodation; and with full power of accommodation in the observed eye 0.5D of astigmatism is readily detected.

The description of the method indicates the readiness with which it may be practiced. In simplicity of apparatus it stands alone. And I need scarcely tell you that in the fixing of astigmatism it saves time as well as gives certainty.

Let me close with this caution: the possibilities of practical value in the shadow test only appear when the details I have mentioned are carefully attended to. It is not to be acquired by the reading of a description, and a few desultory trials, nor will a single successful determination insure that all others will be equally certain and exact. In this, as in anything else, accuracy is only attainable by care and skill.

A CASE OF ULCERATIVE ENDOCARDITIS.[1]

BY GRACE WOLCOTT, M.D.,

OF BOSTON.

On the 17th of October, 1884, I was called to see Mrs. R. M., 40 years of age. She had been a tailoress until her marriage, six years before. She told me that three years previous to this visit she had rheumatism, for which she was treated about one year. She fancied that the remedies given her then, increased the menstrual flow very much at first, and finally arrested it altogether. This so alarmed her that she refused to take any more medicine, and had had no further treatment up to the time that I saw her, although she had suffered greatly with her heart in the meantime. She was a slender woman, rather over medium height. Her face was very pale, her lips bluish, cheeks sunken. She seemed very nervous and her voice was tremulous.

Her pulse was 108, very irregular in volume and rhythm, and frequently intermitted. The radial pulse was so weak that I could not count it accurately, and from this time I was obliged to take the pulse over the heart's apex. Temperature and respiration were normal. The bowels were constipated. The tongue was furred and the breath foul. She complained of gaseous dyspepsia, and of pain over the apex, accompanied by dyspnœa and palpitation, after any physical exertion or mental excitement.

Physical examination showed the apex beat to be in the fifth interspace, three and one-half inches from mid-sternum, and the area of cardiac dulness to be somewhat increased. At this examination the heart's pulsations were so weak and irregular that no diagnosis of the valvular condition was made.

The treatment was directed to the heart itself and the sluggish condition of the bowels, as I thought the dyspepsia was secondary. The bowels were unloaded by a simple laxative, and under the influence of the tincture of digitalis the number of pulsations was reduced from 108 to 80 per minute, the beat becoming stronger and more regular. Auscultation then revealed a double mitral and a double aortic murmur. The dose of digitalis was then reduced from gtt. x to gtt. iii, three times daily, and she was given a pill of cinchona, iron and gentian, and under this treatment she gradually improved. Two weeks from my first visit her pulse was 74 per minute and regular, the bowels moved daily and she was able to go up and down stairs and to take long walks without discomfort.

She continued in this condition until November 26, when after severe nervous excitement, she came to my office suffering from palpitation, dyspnœa and dyspepsia, with a pulse which varied from 116 to 128 beats per minute. Digitalis was again administered and soon all trouble subsided.

On January 12, 1885, I was again called to see her. She had taken a severe cold and had rheumatic pains with constipation, headache, anorexia and vomiting. The pulse was 120, the apex beat very feeble. Her face was extremely pale. She was put to bed

[1] Read before the Section for Clinical Medicine, Pathology and Hygiene, of the Suffolk District Medical Society, May 12, 1886.

and the tincture of digitalis was again given in gtt. x doses, and treatment suitable to her febrile condition was instituted.

January 13. The pulse rate was reduced to 72 per minute, but she suffered greatly from nausea, and passed a sleepless night.

January 14. She complained of severe pain over the præcordia. Urine scanty; specific gravity 10.20; one-fifth albumen by bulk and loaded with urates. A flaxseed poultice was placed over the region of the heart and potass. bitart was administered in small doses every two hours. Sulphate of quinine was given thrice daily and sulphate of morphia at bedtime.

January 15, A.M. The temperature was 101.8°, pulse 76 and respirations 36. The pain over the præcordia was very much lessened but the patient complained of severe pain in the right and left lumbar regions. At this time coarse moist râles were present in both lungs, accompanied by a cough with profuse expectoration of mucus.

January 16. She was rather better and from that date she gained slowly, the convalescence being interrupted only on January 22 and 23, when she had chills followed by sweating. No unfavorable symptoms followed, however, and she was soon able to sit up and her appetite returned. She was given a pill of quinine, arsenious acid and gentian, and the digitalis was continued. She expressed herself repeatedly as feeling better than she had felt for more than two years. She was able to go over the stairs and to walk down town and back. I tried to impress upon her many times the necessity for continuing the digitalis, but she felt so well that she did not see the need of it, and soon after I stopped my visits she stopped all medication.

She had similar attacks on March 7, 15 and 30. The pulse on each of these occasions was 140 but was quickly reduced by digitalis or caffeine. The period from April 4 to April 19 was one of great comfort. She was able to walk long distances every fine day, and to do her housework with perfect ease. At this time I considered all her discomfort to be due to the fact that the limit of compensatory hypertrophy had been reached and dilatation had begun, the crippled condition of the aortic and mitral valves having probably originated in the attack of rheumatism three years before.

With the exception of a slight pericarditis in January the patient had with each attack, bronchorrhœa, dyspnœa and various gastro-intestinal disturbances, all of which disappeared as soon as the heart's action was fully under control, and invariably reappeared about one week after she stopped medication. Repeated microscopic examination of the urine revealed nothing abnormal.

On the morning of April 20, I was called to see Mrs. M., and was told that the previous evening just after going to bed, she had a violent chill, that the chill was followed by profuse sweating, severe pain in the right lumbar region, and that she had passed a restless and sleepless night. At this time, about 8 o'clock A.M., the temperature was 101°, pulse 140 and irregular, respiration 40. Her face was very haggard and anxious. Orthopnœa was marked and there was a copious expectoration of glairy mucus. The prostration was so extreme that the physical examination was necessarily hurried and superficial, but I found on percussion that there was flatness over the lower part of the right chest and on auscultation mucus and subcrepitant râles over both lungs. Her stomach was very irritable and it was with difficulty that she retained any food. She was given one of the ammonia salts together with quinine and morphia.

April 21, there was an improvement and her temperature had reached 103°. I then asked Dr. A. N. Blodgett to see her with me in consultation. A careful examination revealed a slight effusion in the right pleural cavity, œdema of both lungs, and pericarditis friction sounds being present. The possibility of the existence of ulcerative endocarditis was suggested by Dr. Blodgett. No change in treatment was advised, except with regard to stimulants and milk. These had been tried repeatedly, in various forms, but unsuccessfully; however, we renewed our efforts, but to no purpose.

During the 22d and 23d, she remained in about the same condition except that she grew gradually weaker. On the night of the 23d, she went into a comatose state which lasted until 9 P.M., of April 24, when she died.

During the illness, which lasted five days, the temperature ranged from 101.5° to 103° there being marked morning remissions and evening exacerbations. The pulse was 140 throughout the attack and the respirations 40 to 48 per minute. Orthopnœa was present from the first, but she complained of no discomfort or pain about the heart at any time. The profuse expectoration was very annoying to her as she could only raise it with great difficulty on account of the prostration. The skin was cold and clammy to the feel, and sallow, almost cadaveric in appearance. Permission to make an autopsy was given at so late an hour, and the time allowed for it so limited, that great haste was required. For this reason the chest only was opened.

Autopsy. Made twelve hours after death. Body fairly well nourished. Rigor mortis present. The lower half of right lung firmly bound to costal pleura by old adhesions; a few recent adhesions were found. The left lung was entirely free. The right pleural cavity contained about a pint of fluid. The left chest was normal. Slight hypostatic congestion over posterior part of lower lobes of both lungs, and both were œdematous.

The pericardial sac contained about ℥iij of fluid. The heart was increased fully one quarter in size, its walls were hypertrophied and its cavities considerably dilated. On the outer surface of the heart, along the course of the left coronary artery were evidence of inflammation in the shape of little bead-like deposits which were quite firm. Near the apex on the outer surface of the left ventricle was a patch of inflammatory exudation about the size of a small lima bean, which was quite soft. There were several similar patches, though much smaller, at different points of the visceral pericardium, also some cloudiness.

In both articles and in both ventricles were large

ante-mortem clots, the clot in the left ventricle extending into the aorta. The endocardium of the left ventricle showed evidences of recent inflammatory action in the shape of cloudy patches and delicate shreds of plastic matter.

The leaflets of the mitral valve showed fibroid induration, the orifice admitting but two fingers. The ventricular surface of the cusps of the aortic valve were the seat of the ulcerative process. In opening the aortic orifice, the incision was made at the junction of the right and left leaflets next the pulmonary artery, dividing one ulcer, the largest one, which measured three-eighths of an inch by one-fourth. It was of a dirty grayish color, and beneath was disintegrated tissue.

The second ulcer was at the junction of the posterior and right cusps.

A little to the left of the middle of the right cusp was a firm mass about the size and shape of a small pea, with beginning ulceration at its apex.

On the posterior cusp were two similar nodules, one of which was slightly softened at the apex, and the other firm.

There were also evidences of inflammation of the aorta extending about an inch and a half from the aortic orifice.

I am glad to lay before the Section the report of this case, with the specimen, for the criticism of its members. It seems to me that the specimen presents *well*, a gradation of the process from the indurated nodule to the open ulcer.

MEDICAL PROGRESS.

BOILED AND UNBOILED MILK.—The inability, or the disinclination, of the modern woman to supply to her young its natural pabulum, is the most fruitful cause of the high per centage of mortality in infants under two years of age. The ingenuity of man has united with his knowledge of physiology and chemistry to supply a substitute for the natural secretion, and we have in the market a large variety of artificial foods for infants. While some of these are tolerably successful, few, if any, are equal to the milk of the cow, which, with slight modification, and with proper care in its preservation, may be made to quite closely conform to human milk. In spite, however, of the most scrupulous care, raw milk is frequently found to be illy borne. Recent experiments by DR. REICHMANN *(Deutsche Med. Zeitung)* seem to show that by boiling the milk these difficulties may be obviated. The following are the formulated results of these experiments:

1. Boiled milk leaves the healthy stomach more rapidly than an equal quantity of unboiled milk.

2. The digestion of boiled milk is more rapidly accomplished than that of unboiled milk.

3. The coagulation of unboiled milk in the stomach is complete in five minutes.

4. The coagulation is not caused by the acid of the gastric juice, but by the influence of a special ferment (milk-curdling ferment).

5. The acidity of the gastric juice is at first due almost solely to lactic acid, and, later in the process of digestion, to the presence of hydrochloric acid.

6. Hydrochloric acid first appears in perceptible amount forty-five minutes after the ingestion of half a pint of milk.

7. For the first hour and a quarter after the ingestion of milk the acidity gradually increases and then decreases until the milk has entirely left the stomach.

8. The curds of casein, in digestion of boiled milk, are much softer than in the case of uncooked milk.—*Medical Age*, August 10, 1886.

CONNECTION BETWEEN SPLENIC TUMOR AND THE UTERINE FUNCTIONS.—DR. SKORCZEWSKI describes in the *Przeglad Lekarski*, a Polish medical journal, a peculiar case of splenic tumor which had a distinct relation to the generative organs. A woman aged 26, after a difficult labor four years ago, experienced pain in the left hypochondrium, and in the course of a few days noticed a tumor in the abdomen, which rapidly attained to the size it was when first seen by Dr. Skorczewski. It then occupied nearly the whole abdomen, being of an ovoid form, the long axis being transverse. It was hard, with smooth thick edges, and could be displaced upwards to the extent of a hand's-breadth above the symphysis pubis. The urine was normal; also the number of blood-corpuscles. Quinine and arsenic produced no effect. Electricity was then tried, and after a course of the interrupted followed by another of the constant current, occupying together some three weeks, the tumor had diminished so much that when pushed up under the left ribs it only extended about a hand's-breadth below them, The patient's general condition, too, had become decidedly better. Menstruation, lasting four days, then occurred, and simultaneously the tumor again increased to its original dimensions, and the woman's general condition became worse. Neither electricity nor other metnods of treatment which were tried produced any fresh diminution in the tumor or any improvement in the general condition. The connection between the splenic tumor and the functions of the generative organs were evident enough, but no explanation of this connection seems to have suggested itself to the author.—*Lancet*, May 15, 1886.

ANACHARIS ALSINASTRUM.—DR. BRANDES has recently declared the valuable properties of the anacharis alsinastrum, a water-plant which has hitherto been considered as a nuisance. He says that in the district where he lives, and where malaria and diarrhœa yearly appeared in a sporadic or epidemic form, those diseases have gradually decreased since the anacharis alsinastrum began to infest the neighboring rivers and marshes, and for four years have totally disappeared. He proposes that the plant, which came originally from Canada, should be planted in marshy districts, with the view of checking malaria; and the experiment, in view of the evidence adduced in the article under notice, is certainly deserving of consideration.—*Medical News*, August 28, 1886.

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THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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"THE HIGHER EDUCATION OF WOMEN."

Is it for the good of the human race, considered as progressive, that women should be trained and admitted to compete with men in the ways and walks of life from which, heretofore (as unsuited to their sex), they have been excluded by feeling and usage, and largely, indeed, by actual legislation? Such is the question discussed by Dr. Withers Moore in his Presidential Address before the fifty-fourth annual meeting of the British Medical Association. The Address was timely, and the subject one which it was proper that the President of a great Association should discuss; for if the proper answer to the question be the negative, and negative for reasons of public health, it was proper that the dangers should be pointed out by a medical man. Is it for the public good that we should have female doctors, lawyers, divines, professors, Ministers of State, astronomers, mathematicians, generals? Were women intended for work of this kind by nature? Is such work of special detriment to them as well as to the public good?

"The first requisite to success in life," says Mr. Herbert Spencer, "is to be a good animal; and to be a nation of good animals is the first condition to national prosperity." To be a race of good animals is also, as Dr. Moore points out, the first essential of human well-being. To be a good animal means a great deal. We often hear it said that we do not so much want a very robust health as great intellectual attainment and intellectual strength; that it is the mind, the brain of a nation which makes it; and that in the struggle of life the body must take its chance, but come what may the mind must be pushed to the utmost. It would of course be a waste of time to argue such a question before intelligent and thinking medical men. The question which Dr. Moore has asked and discussed is one that has a peculiar interest for the American. We are a people in a hurry, and we take but too little interest in what our neighbor is doing so he does not interfere with us. Many of our aims and ends are attained at an expense of nerve and muscle force which is unknown to other nations. But that is not because we are deficient in brain-force; it is because we do not pay sufficient attention to the proper development of the body that must sustain the brain. Our breed of men must be bettered, and the first requisite to this end is to have the breed of women bettered; good male animals cannot be reared without good female animals. Law and custom have, from time immemorial, imposed certain restraints upon women; are we thoughtlessly to throw aside these restraints and place women in competition with men? When we ask the question, Is the preliminary training for such competitive work, and the subsequent practice of it in the struggle for existence, of such a nature that it will more or less indispose and incapacitate women for their proper sphere, their proper function, of being mothers of men? we must look for the answer from those who can give a practical answer—no theoretical or sentimental people can give a correct answer.

One of the best answers to these questions may be found in the first chapter of Emmet's work on "Gynæcology," pages 17 to 25. It is a chapter which should be reprinted and placed in every family and school in the land. One effect of this higher education of women, of which we hear and read so much, will be to hinder those who should be good mothers of men from being mothers at all. Good, the best, women cannot take the place of good men; they may do some of the work that would have been done by the sons, but they cannot do *the* work that the sons would have done under any system of training. A good professional education is very expensive; the expenditure is not alone one of money, but there is an enormous outlay of physiological force; much more than a woman can afford, if she is to perform her proper functions as a producer of men. Clearly that is the function of woman, for she is the only means by which man can be brought into the world. The production of children calls for far more physiological force on the part of the mother than of the father. The functions of gestation and maternity require a great outlay of physiological force, and if this force is used up in other work the offspring of the world must suffer, as must the woman herself.

We have, then, two channels of expenditure of physiological force of the woman: the terrible strain of higher and professional education, the training for competition with men in the most severe exercises of the intellect, and the expense of being properly trained for motherhood. It is of the greatest importance to a man, to his country, that he should have had a good, an able mother, a strong mother; a healthy, strong animal, as well as an intellectually strong human being. More than being mothers of men, women are the mothers of mothers of men, of the material of which men are made. "Material," says Dr. Moore; "why the word itself is but the Latin of mothering, a lengthened out form of *mater*. We have no corresponding *paterial*." Not only is woman the producer of this man-material, but she has charge of its shaping and moulding during the most plastic period of its existence. But "instead of nine or ten years forward, from his beginning on the day on which he was born, go nine months backwards, to the true beginning and starting-point of him, when, by growth and development, he had to be built up and prepared for being born. The whole ante natal structure, with all its complexities, is merely and exclusively made out of the mother's living substance; it has been subjected to influences and impressions exclusively maternal; it is mother-stuff throughout. And this goes on thus all the time during which it is developing into readiness to become the new-born babe—the man that is to be. Blood and bone, nerve and muscle, are all made out of mother-stuff. Can we exaggerate the importance to the future man of the quality of the mother-stuff?" And is it not equally important to the future woman, the future mother?

"Educate a woman, and you educate a race." This saying is full of promise if it be rightly interpreted; full of dire disaster if it be applied to the mind to the exclusion of the body. While it may be true that too much bodily labor may render women less prolific it is very much more clearly shown that excessive mental labor is a cause of sterility (or infertility). "In its full sense," says Herbert Spencer, the reproductive power means the power to bear a well-developed infant, and to supply that infant with the natural food for the natural period. Most of the flat-chested girls who survive their high-pressure education are unable to do this. Were their fertility measured by the number of children they could rear without artificial aid, they would prove unable to do this." It would be a curious and a not uninstructive task to take a census of the nursing bottles used in the various civilized countries. It would be a tolerably accurate index of the animal-power of the women in those countries. It is extremely probable that twice as many are in use in America as in any two or three countries of Europe; and when we come to compare this country with China, for example, it is safe to say that we destroy more nursing bottles each year than have been used in China since the instrument was thought of. Our education, our civilization, is one that stunts, deforms and enfeebles women; and as such it must be unsound. Shall we make it worse by placing women in competition with man, by still further stunting, deforming and enfeebling her animal powers? The goal to which we are going is to have the weaker and inferior woman perpetuate the race; the very ones who, if any, should be prohibited from producing men and mothers of men. If it were at all necessary in the interests of human progress, if it were likely that the world and mankind would be the better for it, there would be an excuse for a thorough and honest trial of it; but there is neither necessity nor justification. And this is more applicable just in proportion as the women under consideration have had unfavorable antecedents as to food, rearing, and general surroundings. At best they are scarcely fit for reproduction until the animal has been more fully developed; this not done, and the immaterial animal is developed to the disadvantage of animal powers, and they are fit for nothing. Instead of development there is degeneration and impaired nutrition, and evolution gives way to nervous and bodily dissolution.

Whatever does, or tends to, render women infertile is prejudicial to a nation. The sterility of its women caused the downfall of the Roman Empire, or was one of the principal causes; among modern peoples the French are giving the world an example of decadence which is largly due to infertility of its women. Every woman cannot be a mother, or a wife; no more can every child grow to adolescence. But it is not assuming too much to say that woman was born to marry and bear children. In England, for example, there is an excess of marriageable men. This is to be attributed to England's standing army; the husbands of those unmarried woman may be found scattered and buried all over the earth. To say that if every woman was a wife it would involve many unions worse than solitude is entirely outside of the question which we are discussing; that people will contract foolish marriages is no argument against the principle of marriage. We cannot agree with our contemporary, the *British Medical Journal*, in its Hibernianism that "if every English woman were employed as a wife, there would still be a consider-

able surplus who would have to employ themselves." There are some women who must work, and if they cannot find woman's work to do then they must work as best they can, even if as men. But there is now a tendency to turn a woman into a man as a sort of experiment or *fad;* a young girl, unthinkingly perhaps, takes it into her head that she must do man's work, though there is no necessity for it, and when she has not the constitution to do a woman's proper work. Parents, teachers, and the family doctor should endeavor to repress this growing tendency. If our girls were more properly taught at school, if they were taught the true physiology of woman, of life itself, this tendency would be in a great measure stopped. We cannot turn man into a woman, nor fit him to perform a woman's duties; no more can we fit woman to perform the work and duties of a man.

THE REMOTE EFFECTS OF REMEDIES.

The Addresses in the Sections of the British Medical Association this year were of an unusually brilliant character, and without invidious comparison it may be said that none were more interesting than that in the "Section of Medicine," on "The Remote Effects of Remedies," by DR. W. H. BROADBENT. As it is necessary to have in view in the treatment of disease the remote effects of the disease, so we should look beyond the immediate effects of the treatment, of the remedies used, to the remote effects. If we give morphine or bromides to-day what will be the effect to-morrow or the next day? If we use them all this week what will be the effect next week or next month? Will not the remote effects be different on different persons, and wherein will they differ? We may use alcohol, for example, with safety as regards future effects in one case, but in another these effects will far outweigh the possible benefits. We may administer chloral for the purpose of inducing sleep, but should the patient depend on the drug instead of taking the proper exercise, and get a proper amount of fresh air, the remote effects of the drug will be as bad or worse than a temporary insomnia. Equally bad are the remote effects of cocaine when a person of weak will once finds that its use will bring activity, exhilaration, and excitement. It may be said that the injurious effects which follow the continued use of a drug are the result of the patients taking the remedies into their own hands, of abuse rather than use; and this whether the patient has at first undertaken to treat his own case, or whether the drug has been first prescribed by a physician, or its use sanctioned by him. Of course there are some cases in which the remote effects cannot be clearly seen; but it is well to be on our guard.

In treating a case of gout, for example, we can cut short the attack by the free administration of colchicum, veratria and other drugs of similar action. The injudicious use of the drugs may cause immediate prostration, and the remote effects may be very disastrous. But the violent pain of acute gout warrants us in using colchicum, but on the condition that proper precautions are used to prevent the remote effects; we institute an after-treatment with aperients, alkalies, iodides, and careful regulation of the diet. The after-treatment, especially the strict diet, is unpleasant to the gouty patient, and is frequently disregarded. It is difficult to make him see that the after-treatment will do what the attack of gout would have done—clear the system of the gouty poison, and bring the secretory and excretory organs into a state of functional activity. On the assumption that gout is due to the formation of uric acid in the system, it is not supposed that the use of colchicum will arrest this formation, it only checks antecedent metabolism, and then nitrogenous waste rather than urea and uric acid accumulates in the system. They provoke resistance in the arterial tree, and then comes increased arterial tension, with all its dangerous consequences. All the consequences of increased arterial tension—vascular and cardiac troubles, arterial degeneration, cerebral hæmorrhage, aneurism, cardiac dilatation, and valvular disease—may be prematurely induced by means taken to keep down or prevent an attack of gout. This is very readily seen in workers in lead, which, as we know, is a common cause of gout when accumulated in the system. Painters and plumbers take all kinds of gout specifics, the attacks are suppressed or prevented, the system becomes charged with imperfectly oxidized matter, and we see as a result deposits of urate of soda in the small joints, the arteries injured by degenerative changes, due to high pressure; or the heart may be dilated by resistance in the peripheral circulation; granular disease of the kidneys comes on, as a result of the state of the blood, or of the vascular tension to which the state of the blood has given rise; the renal lesion often aggravating the bad state of the blood. In this state the system is easily overcome by diseases which under other circumstances it would safely weather. The "Banting" system of treating obesity may also cause high arterial tension. The highly nitrogenized diet, the exclusion of non-nitrogenous substances, and the limitation of the amount of fluid taken, gives a disproportionate amount of nitrogenized matter in the

system, while there is not the amount of water necessary for active metabolism and elimination. A part of the system is a large amount of exercise, and it is a very necessary part, but it is the very part which is most frequently neglected.

In some of these cases it is the patient himself who is wholly responsible for the consequences, interfering ignorantly with natural processes, but medical men are sometimes responsible for very similar errors. Dr. Broadbent thinks that "a similar error, for which medical teaching is directly responsible, is committed when strict dietetic treatment is insisted upon in the glycosuria of stout elderly gouty subjects. I will not stop to discuss the question whether this is to be called diabetes or not; but to be quite clear, I may state that the cases I have in mind are not those in which there is merely, from time to time, a small amount of sugar in the urine, but such as have copious urine with high specific gravity, and the constant presence of much sugar, often with thirst. Now, in some of these patients the establishment of habitual and free elimination of sugar is coincident with the disappearance of many distressing symptoms, especially digestive troubles, loss of appetite, irritability of temper, disturbed sleep, and morning depression. "The altered chemistry of the liver is, in some way, a relief; perhaps it is by the increased elimination of the urea which accompanies the sugar, the two coming from the splitting up of nitrogenized matters. I do not wish to argue from this, that we must let the glycosuria alone, but only that we mnst bear it in mind in our treatment. Now, in my experience, this form of glycosuria is almost always associated with high tension in the pulse, which is not constantly the case in the diabetes of early life, the pressure in the arteries here being often extremely low; as everyone knows also, it is common in gouty persons, and it may or may not appear to relieve the gout; very often it does nothing of the kind. It appears to me that, with all these evidences of the presence of nitrogenized waste in the blood, it can scarcely be right to insist on a diet from which non-nitrogenized matters are strictly excluded. It may be added that, as a matter of practical observation, such a diet is rarely, if ever, adhered to; patients belonging to this class are not usually such as have been accustomed to self-restraint, or such as can resist the solicitations of a newly reviving appetite; and, when it is attempted, do we not find the mouth to become foul and dry, the appetite fail, the bowels deranged, till we are compelled to relax the restrictions?" In some of these cases the heart suffers, or has already sustained some injury, and the long full pulse of sustained tension has given way to the short sudden wave in a large full artery—a sign of dilatation which examination of the heart will confirm. These are the cases which are so much benefited by the eliminant action of large quantities of water, particularly when certain saline matters are found in the water. There is direct proof that elimination is the therapeutic indication. The health of the patient is what we are after in these cases, more than the removal of all the sugar from the urine. Under an eliminant treatment the patients feel and are better, with a modified mixed diet, than on what is called a strict diabetic diet. And more than this, under the eliminating action of a blue and colocynth pill, once or twice a week, followed by salicylates and alkalies, the sugar often disappears from the urine for months and years.

The continued use of the solanaceous drugs for asthma, especially in cases of catarrhal asthma, and of certain "asthma cures," while at first bringing relief to the suffocating patient, will in time produce such effects on the vascular and nervous systems, stupified mental condition, congested face and eyes, the large sluggish pulse of arterial paralysis, dilatation of the right heart, that they must be discontinued or they themselves will kill the patient. Probably no one affection is so persistently maltreated as sick-headache; and almost on a par with this may be placed the so-called indigestion. There are very many cases of each, or both combined, which call for eliminant and tonic treatment, but which do not get it. In indigestion it is often the case that the stomach must be immediately built up to the food, instead of putting the food down to the low level of the stomach. In the first it is often imperative to build up the blood and nervous system, the heart itself, so as to keep up a state of constant elimination of effete matter. "It is quite true that most people eat far too much, and, again, that with regard to the stomach, as well as to all other organs and parts of the body, the principal of functional rest is of primary importance in dealing with disease; and restriction of food, and even temporary starvation is often necessary; but we must distinguish, and not starve, those who are suffering from inadequate nourishment, or employ treatment for catarrh, or ulcer, or organic disease, when nothing of the kind is present."

THE INGLESBY LECTURES.—On September 2d and 3d MR. LAWSON TAIT, President of the British Gynæcological Association, delivered two lectures on "Recent Advances in Abdominal Surgery," in the Queen's College, Birmingham.

SOCIETY PROCEEDINGS.

CHICAGO MEDICAL SOCIETY.

Stated Meeting, August 16, 1886.

E. J. DOERING, M.D., PRESIDENT, IN THE CHAIR.

DR. F. E. WAXHAM presented a

MEMBRANOUS CAST OF THE TRACHEA AND LARYNX.

The specimen presented was removed from a child nine years old. This cast had remained in the larynx and trachea for several months. The patient was brought to me from Minnesota by Dr. McDavitt, of Winona. The history is as follows: The child claims that in April she swallowed a hedge thorn while away from home. She was at once taken with suffocation, and twenty-four hours afterwards was operated upon when at the point of death, by Dr. McDavitt, who performed tracheotomy. The child was unable to breathe through the natural passages after the introduction of the tracheotomy tube, although many attempts were made to remove it. It seemed impossible for the child to get a breath through the natural passages when she was brought to me. Upon laryngoscopic examination the larynx seemed to be closed, but digital examination revealed a very small opening into the larynx. In this opening a small sound was passed and this was followed by one of the smallest size intubation tubes, this by a larger one, and finally the largest size tube was introduced. It could not be passed on account of the tracheotomy tube, and violent vomiting ensuing the tube and this cast were ejected. After the rejection of this membrane a large sized tube was introduced and pressed down into position, as the tracheotomy tube was removed, which gave the child perfect comfort. She remained comfortable after the introduction of the tube, took several glasses of milk during the afternoon, and in the evening was taken to the train and returned to Minnesota, the intubation tube remaining in the larynx to be removed by the doctor in the course of a few days.

DR. JOHN B. HAMILTON, of Washington D. C., read a paper on

THE RADICAL CURE OF INGUINAL HERNIA,

(See page 256.)

DR. D. W. GRAHAM said: I think I express the sentiments of every member present when I say that it is probably not possible to find in the English language as complete and satisfactory a review of the history of the various operations for the radical cure of hernia as Dr. Hamilton has given us in this paper. Certainly it has been gratifying and instructive to us all to listen to it. Some of the figures which the Doctor quotes give us an idea of the great prevalence of hernia. I believe reliable statistics show that on an average about every fifteenth or sixteenth individual in civilized communities suffers from some form of it. When we remember this great prevalence, and when we remember that every subject of a hernia sustains thereby a certain amount of disability, and that very many are entirely disabled by it, the subject of the radical cure of hernia assumes an importance not always accorded to it. I think the author of the paper takes rather a highly colored view of the future of these operations. It is not probable that this generation, at least, will be able to use the expressions he thinks surgeons will be using some day in Chicago. However, this degree of success is to be looked forward to and attained if possible. In regard to the modern methods of operating, I understand the distinctive features of Wood's method to be that it is almost entirely subcutaneous, that he uses wire, and that he allows the sac to remain in the canal as a kind of plug. So far as I know, this method is not practiced in this part of the country to any extent. In Mr. Wood's hands it seems to be successful in permanently curing a considerable majority of those operated upon, and in decidedly benefiting a good many who are not permanently cured. His statistics show that there is almost no danger to life from the operation itself, at least in his hands. Any method which will give such results and at the same time involves so little risk to life, must be a good operation.

The open method, the one chiefly practiced in this country, according to my observation, contemplates strict attention to antiseptic details and the avoidance of suppuration. It involves a little more risk to life from the operation *per se*, but to compensate for this it would seem to give, theoretically any way, a larger per centage of permanent and complete cures. The modifications or varieties consist chiefly in dealing with the sac. Although MacCormac claims that unless the rings are wide there is no advantage in attempting to close them. It seems to me the best way to treat the sac is to excise a section of the neck between two ligatures, pushing the stump into the abdominal cavity and allowing the body of the sac to remain, unless it is small and loosely adherent, when it may be extirpated. After any of these operations there remains the funnel-like depression on the inner surface of the abdominal wall, which favors a recurrence of the hernia however efficiently we may have obliterated the sac and closed the rings. MacEwen, of Glasgow, in a recent article describes and advocates a plan he has devised for obliterating the depression. He utilizes the sac by dissecting it from its surrounding attachments, putting a suture through it from side to side beginning at the lower end, thus making a corrugated pad, which he pushes through the internal ring into the cavity, after first separating the peritoneum for a little distance aroung the ring with the finger. This makes a convexity on the inner surface of the abdominal wall, when there would otherwise be a concavity. This modification appears to be a real improvement, but the practical value of it is as yet largely conjectural. By way of adding something from an historical standpoint, I might mention, what I think was not alluded to in the paper, that electrolysis has been used and advised to set up a plastic inflammation in the inguinal tract. I believe this suggestion comes from a Cleveland surgeon, whose name I have forgotten. This is the same in principle, of course, as the use of subcutaneous injections, and I should think

would be preferable to the injections, if I were to judge of it without having tried it.

DR. MOSES GUNN said: This subject is of intense interest, and we are very much indebted to Dr. Hamilton for the exhaustive review he has given us. We have to consider what are the best methods for operating with the intent of effecting a radical cure. I am inclined to discard all the old invaginating processes. In the first place, the invaginated portions are always liable to subsequent prolapse; in the next place, it is always a foul mess. It won't do to simply remove the cuticle; all the organs of the skin must be destroyed. The skin contains the hair, sweat and sebaceous follicles, and unless they are destroyed, they will continue their work, and thus accumulate material for decomposition. In order to make a success, you are obliged to do more than destroy the cuticle. You will have to destroy the skin, and that is a very slow process and exceedingly nasty. Therefore I am inclined to repudiate all invaginating processes. I repudiate, also, all of the subcutaneous processes, for they are blind procedures, and I would not adopt them where an open operation could be as well, and even more advantageously and safely resorted to. I believe the best and surest method of trying to effect a radical cure of hernia is the open method. This method should be performed in every case where an operation is made for the relief of a strangulated inguinal hernia. Just as soon as the operator has opened the neck of the sac and restored the prolapsed viscera or viscus, he should close up the wound. He should begin at the topmost portion of the sac, and with a curved needle and catgut take it up and ligate it as near to the internal ring as he can. Then he should drop down about half an inch lower and ligate again, and so on down through the canal. He should then approximate the pillars at and above the external ring as closely as possible and close the outside tissues. Thus the operation which is made for the relief of the accident should be made an operation for radical cure. It can be done more effectually at that time than any other.

Then again, the physician is called upon to make an operation for a radical cure; the patient comes to him complaining of the truss, which has become ineffectual, the hernia escaping in spite of it, and his life becoming a misery to him, and he asks if something cannot be done to make a radical cure. The answer is, "yes;" but the question is, by what method shall we make it? I say by an open operation, practiced with all antiseptic precautions. Cut down upon the parts, separate and dissect out as well as you can the neck of the sac, the hernia having been reduced; ligate the sac and cut out a portion. Thrust the stump back into the canal and approximate the pillars, closing them tightly and keeping the external ring tightly closed. So much for the method; now for the prospect of success. How much right have we to expect what might justly be called a radical cure? By the term radical, I mean permanent. In what proportion of cases can we expect to have a permanent cure, so that the patient will never have hernia again? What is hernia, and who have it? I think I can safely say that the typical man never has hernia. When the true type in development has been attained and the abdominal walls closely woven together, they are proof against such accidents and there will be no hernia. Hernia is the result of the imperfect development of the abdominal muscles and aponeuroses. When that imperfect anatomical development obtains in the patient, the abdominal muscles are thin and flabby, and in such cases we get hernia. Nor can we in such a case by an operation make a man better than his Creator made him, but if we can make him as good as he was we may congratulate ourselves. After we have operated and closed up this weak point as well as possible, the very best result that we can expect is that we have made the man as good as he was before he had hernia; but, if he was weak enough to have hernia from certain exciting causes, he will be weak enough to have it under similar circumstances again. If his hernia is brought on by lifting and straining, the same exciting cause will bring it on again, and under these circumstances a radical cure is only measurably radical. We should tell the patient after operation that if he will be more careful and take no violent exercise, he may hope for exemption from hernia.

In other cases, the patient has an old and immense hernia and cannot wear a truss; we operate upon him and can say to him that if he will be more careful and wear a truss, he can be tolerably comfortable for the rest of his life. Such, I apprehend, is the true aim and scope of operations for the radical cure of hernia; such are the precautions we should give our patients, as they must become our coöperators in order to make this operation a success; and with such coöperation and conscientious efforts on our own part, radical cure of hernia becomes a standard and important operation, as important as the subject itself, which, as we have seen, is of immense importance on account of the great dissemination of the disease.

DR. E. F. WELLS said: Dr. Hamilton has certainly read a very interesting and extensive paper. There is one point in particular mentioned by the author, namely: that he advocates an operation in all suitable cases where an operation is not distinctly indicated. Every practitioner of large experience must certainly have met with many cases in which the truss has been applied resulting in a cure, and I think the truss should not be stricken entirely from the radical cure of hernia.

DR. HAMILTON in closing the discussion said: I need not say that I am extremely gratified to find such substantial unanimity of sentiment as to the propriety of operation—nay, as to the necessity of operation, but there can be no doubt as to the necessity of further statistics on the subject. In regard to invagination, I think a reading of the paper will not show that I advocated the method of invagination recommended by Gerdy. In the recent open method there is no invagination. Under the original Wood's method, the subcutaneous fascia only was pushed up under the ring; by the open method we cut down directly on the sac. This open method is really a combination method, because it brings together the pillars and takes care of the sac. Statis-

tics are necessarily unreliable as to the ultimate permanency of the cure of these cases. The best statistics are those shown by the Swedish Hospital, where out of 300 cases a large percentage of recoveries is shown, and if statistics are worth anything in determining the success of a method, we must place some reliance on these. It would be well to have patients come back every year for the purpose of re-examination.

Dr. Fenger, if I correctly understood him, speaks of the influence of suppuration in curing these wounds by letting them heal from the bottom, but in the various subcutaneous operations that is exactly what it is intended to avoid. There is no doubt that suppuration will make a radical cure of hernia if the patient's strength lasts, and the suppuration does not extend into the abdominal fascia. That was the method by which the old red-hot irons accomplished their purpose. The mineral acids produced a radical cure by the destruction of the tissue and healing from the bottom. The seton also performed a cure, but it has so many disadvantages that it is not to be compared with those procedures that stop the inflammatory processes short of the decomposition or death of the exudate. In regard to operating on children, I think the argument cannot be regarded as sound that we should not operate on them on account of the difficulty of keeping a bandage on, for surely if any cases are to be benefited by an operation for radical cure, they are those in which the patient is young enough to grow—in which the tissues can be brought together and retained with great hope of a permanent cure. Everybody knows that cases do recover by the use of the truss, but the proportion, I believe, is less than by any other method. As stated by Professor Gunn, it is found that a majority of operations for strangulated hernia are, in effect, really operations for the radical cure, and there are more than five cures from operations to one after application of the truss. And when we remember that there are 250,000 trusses manufactured per annum in Philadelphia alone, I doubt very much if it can be shown that trusses have even a fair percentage of recoveries following their use.

Dr. Wm. T. Belfield reported a case of

SUPRA-PUBIC CYSTOTOMY WITH EXTRACTION OF LARGE CALCULI, AND CORROSIVE SUBLIMATE POISONING.

The patient was a feeble, emaciated man, 71 years old, who for nine years had suffered from cystitis of steadily increasing severity, caused, as was supposed by his various physicians, by prostatic enlargement. He had been sounded for stone a year ago under chloroform, but with negative result. For two years he had been unable to empty the bladder except by catheter. He refused permission to introduce the sound because convinced that he had no stone, but was anxious to have an operation for the removal of the prostatic enlargement.

June 7, supra-pubic cystotomy was undertaken for the purpose of removing by galvano cautery that portion of the prostate which was assumed to project into the bladder. The introduction of the sound under ether revealed a large stone; the usual incision was made; the finger in the bladder found two calculi, one behind the prostate, the other adhering to the fundus of the bladder, each about as large as a walnut. The first stone was crushed and the second with much difficulty removed entire. The patient was so collapsed that no attempt was made to remove the prostatic outgrowth, which could have been accomplished without much difficulty, and the following day the temperature was 100.5°, the highest observed. On the third day it was normal and so remained. The wound was irrigated once daily with a bichloride of mercury solution. The progress was entirely favorable until the eleventh day, when there began a severe diarrhœa with much rectal pain and tenesmus, and later the evacuations were tinged with blood and the patient complained of a metallic taste. Sublimate poisoning was recognized and the solution discontinued. Temporary improvement followed, but death ultimately resulted on the thirty-sixth day after operation. No autopsy was permitted, but a hasty examination of the abdominal contents was made. Peritoneum, kidneys and bladder were normal, except that the latter was much hypertrophied. The intestines could not be opened; the calculi weighed 2 ounces and 6 drachms.

THE DEATH OF DR. F. H. HAMILTON.

Dr. Truman W. Miller offered the following resolution, which was adopted:

Whereas, This Society has learned with deep regret of the death of Dr. Frank Hastings Hamilton, of New York, and

Whereas, In his death the United States has lost one of its most distinguished surgeons; one of its ablest teachers; one of the purest patriots, and, in his private life, one of the most amiable of men; therefore, be it

Resolved by this Society, that we hereby give public testimonial to the many virtues of the deceased, and that we tender his family the assurance of the profound sympathy of this Society with them in the hour of their affliction. And that a copy of these resolutions be furnished them, and a copy spread upon the records.

SUFFOLK DISTRICT MEDICAL SOCIETY.

Section for Clinical Medicine, Pathology and Hygiene.

Stated Meeting, May 12, 1886.

Dr. F. L. Knight, Chairman.

Albert N. Blodgett, M.D., Secretary.

Previous to the papers of the evening, the Chairman announced that Dr. Bowditch desired to address the Section.

Dr. Bowditch said that he had three articles which he would present to the Section, two of which would be hereafter placed in the portfolio of the Boston Medical Library. They all held important relations with the history of medicine, and he thought they would be interesting to the members.

First. A splendid lithograph of Vesalius, "The

Father of Modern Anatomy." No one could look at it without feeling that if Vesalius had not been great in our profession, he would have been great in any walk of life. One could believe from his fine commanding brow and calm look that he might have been a statesman. He was born in Brussels in 1514, and died in 1564 of starvation in the Island of Zante, where he had been shipwrecked on his return from a pilgrimage to the Holy Land. Accused by the minions of the Inquisition of having begun to dissect a man while the heart was still palpitating, he was saved from death only by the favor of Philip II, to whom he was chief surgeon. He had taught anatomy at Pavia, Bologna and Pisa from 1540 to 1544, and had large crowds of students to hear him. His immortal work, "De Corporis Humana Fabrica," in seven volumes, was first printed at Basle in 1543. Boerhaave and Albinus published another edition in folio in 1725 at Leyden.

Second. A pharmacopœia published at Cologne with Petrus Holzeming as editor, a worthy man doubtless, but now wholly unknown to fame, so far as biographical dictionaries are able to tell us. It is a thin folio volume, with large, well engraved title-page. At the top are the armorial bearings of Cologne. Below is a view of the city. On the side, are four physicians, namely: St. Luke, St. Parataban, St. Agmar, and St. Damianas. The last three suffered martyrdom in the fourth century. St. Connes was for many years the patron saint of physicians. A church dedicated to his memory in the thirteenth century was taken down in 1832. At one time the fraternity of priests of St. Connes taught medicine there. Ambroise Pare in the sixteenth century gained his title of Master Surgeon from that Faculty. The pharmacopœia is a most interesting work, as illustrative of the absurd lengths our fathers in medicine went in the number of articles put in their prescriptions. Generally speaking there were one or two active ingredients, as aloes, opium, hyoscyamus, etc., in a prescription, and the remainder were inert. The most remarkable in the book is an electuary powder. It contains fifty four ingredients and was styled "Pulvis Subtilissimus."[1] In this prescription, human nature had evidently gone to the utmost extreme of over-medication. The whole book with its absurd remedies seems to prophesy homœopathy, with its single and equally absurd infinitesimals. Man never goes toward the truth in a straight line, but in a pendulum course.

Third. A photograph of a painting by Watteau, caricaturing in pencil after Moliere had done so with his pen, the absurdities of the faculty and the extremes to which the public were led by eulogiums on the use of the "clystere." Moliere wrote his "Malade Imaginaire" for the opera of the king and noblesse. Watteau, coming after him a few years, painted the scenery on the same stage. The painting which the photograph represents is in the museum at Lille, in France. It shows an immense army of people coming to meet the great Doctor, who is dressed in fur-bordered robe, with feathers in his hat. He stands erect with immense gravity and an important look, his right arm extended as if giving decided directions in regard to a patient who is about to have an enema given to him. An immense crowd looking like an army covers the background, each person armed with a huge syringe, instead of the usual trappings of war. Upon the top of the staff, bearing the color of the regiment, instead of the imperial eagle appears an upturned chamber utensil which shall be nameless. Various groups are seen in the foreground, of a most realistic character. A lady with glutei and legs exposed, and two negroes administering an enema. A lady with her lapdog on her arm and her husband are apparently leaving the scene, but are followed by an attendant of the doctor, armed with a syringe, who is evidently entreating her to allow him to give her an enema. On the other side of the picture is a burly postman, set upon by two attendants, one of whom has pulled the hat over the eyes of the struggling victim, while the other is preparing to give him the injection, which one sees the postman will not readily submit to. But most comical of all is a poor, evidently costive dog, with curved back, plainly striving in vain for a dejection, and mutely appealing for an enema for his relief. One has only to read the "Malade Imaginaire" of Moliere, to get a full explanation of this gross caricature. At the same time it shows the absurd length to which our fathers went in regard to the rectum, as according to the pharmacopœia above described, they had a half century or more before, filled the mouths of their patients with their subtle powders. Who can wonder at homœopathy and eclecticism in consequence of such nonsense.

[1] Duke Don Nicholas, so called because prepared by an abbot for the son of Don Nicholas.

DR. GRACE WOLCOTT read a paper on

A CASE OF ULCERATIVE ENDOCARDITIS.

(See p. 264.)

DR. F. C. SHATTUCK, upon being invited to open the discussion, expressed his interest in the well reported case which had just been read. The pathology of endocarditis in general presents problems at once interesting and difficult, to the solution of which we thought ourselves nearer some years ago than we do to-day. Until lately the real distinction between ulcerative and simple endocarditis was thought to lie in the presence in the former of micrococci, and the term diphtheritic endocarditis was proposed to characterize the process at one time. To these organisms the infectious nature of the affection was attributed; to them it was thought to be due that emboli excited suppurative inflammation about their points of lodgment. This distinction now fails us since Klebs, Köster and others find micrococci in the products of any and every endocarditis, without having succeeded as yet in distinguishing between those accompanying forms which are clinically so different. Anatomically the difference is rather one of degree than of kind, and hence Osler prefers the term malignant to the term ulcerative. In an endocarditis of malignant course there may be no more ulceration than in a benign case of warty endocarditis; and, on the other hand, there may be well-marked ulceration from very different causes. The main anatomical distinction is

in the different results of embolism; as to the pathological distinction we are at present ignorant; the clinical distinction is more clearly marked. Thus we are led to the eminently practical question of diagnosis. In some malignant cases the diagnosis may be made with tolerable certainty. We know that the process is especially liable to attack valves which are the seat of changes dependent on previous disease, notably sclerosed valves; and hence grave typhoid or pyæmic symptoms, for which no other cause can be assigned, in an individual known to be the subject of old valvular disease are, to say the least, suggestive. Similar symptoms with indications of embolic abscesses in a case of acute rheumatic endocarditis are, in like manner, suspicious; but this association is, according to Osler, rare. In cases of primary malignant endocarditis the diagnosis may be reached by a happy guess, but is generally beyond our powers. The general symptoms vary very widely in different cases, simulating typhoid fever, pyæmia, malaria, or some other general infectious disease; the leading symptoms may be cerebro-spinal; symptoms pointing to the heart may be wanting or excessively obscure. If the heart was previously healthy the organ is not enlarged and the course of the disease is usually too short to allow it to become so; while a souffle can easily be attributed to so-called dynamic causes. A case which recently occurred at the Massachusetts General Hospital, in the service of Dr. Abbot, the autopsy on which the speaker attended, well illustrates the impossibility of diagnosis at times. A woman died after an illness of ten days or so, with ill-defined but rather typhoidal symptoms and no indication of cardiac disease. Primary ulcerative or malignant endocarditis limited to the tricuspid valve with emboli in the lungs was found. The speaker had seen two or three cases in life in which he had suspected this form of disease, but had not been so fortunate as to secure an autopsy to overturn or confirm the diagnosis. He congratulated Dr. Blodgett both on the diagnosis in the case just reported and on its verification.

Dr. E. G. Cutler expressed much interest in the case reported He had never seen an instance of ulcerative endocartis at autopsy, but had had, a few years ago, a patient under his care with the following history; a child of 6 had passed through an attack of mumps, under another physician; two other children were sick with the disease about the same time. Some days afterwards the child, having been all the time ailing, was seized with chills, fever, pain in the præcordia, and restlessness. There was found to be a loud systolic murmur at the apex. The patient got worse, the circulation in one of the arms suddenly ceased, accompanied by pain and swelling. The femoral artery in one of the legs became plugged soon after, associated with severe pain. The murmur ceased before death, which occurred about a week after the patient was first examined.

Dr. H. I. Bowditch stated that he had never seen a case of this disease in his life, and that the condition was unknown to him. The specimen was a most interesting one, and showed a new source of danger in diseases of the cardiac tissues. Dr. Bowditch asked if the diagnosis of ulcerative endocarditis were made before death, and if so, on what grounds was this opinion based?

Dr. Blodgett replied that he had the privilege of examining the patient before death, and from the history of a long-continued organic disease of the heart which had been studied by careful physicians for many months, to which a new and most serious complication had been suddenly added, he was led to review the conditions to which this unexpected change might be due. The character of the symptoms observed at this time was essentially that of acute septicæmia; that is, there was acute pain in various parts of the body which were not before the seat of disease. The pulse and temperature were very high, the patient was in a condition of stupor, almost amounting to apathy, and resembled the state called "typhoid" in many asthenic diseases. The array of symptoms thus calling attention to a condition so closely approximating to that of surgical pyæmia, it seems only a short way to the diagnosis which alone would sufficiently explain the symptoms. The autopsy was of necessity very hastily made, and gave no time to examine the other visceral organs, which might possibly have shown other interesting pathological conditions connected with the state of the heart.

Dr. Shattuck said that in any case of endocarditis we may have embolic formations in the blood-vessels. In benign endocarditis the emboli do not occasion suppuration, while in the form of endocarditis which we may for the sake of comparison call "malignant," the emboli are followed by suppuration, and the appearance of a train of clinical phenomena bearing considerable resemblance to septicæmia.

Dr. Bennett F. Davenport spoke of the

PHYSICAL AND CHEMICAL QUALITIES OF ORDINARY COWS' MILK,

and added: The question of the age of the milk which is given to our city children I consider as of very much greater importance than it is usually deemed by physicians. There is no fluid that I know of which is a more rapid absorbent of foul odors of all kinds, or which offers a more fertile field for bacterial growths. Yet most of the milk, even when just delivered to the consumers in this city, is already in its third day of age, and has been exposed to a probability of contamination so great that I very much wonder that it does not become downright soured before the time of the next daily delivery. Very much of it is but little short of this stage. I am firmly persuaded that it is to this changed condition in the milk, to which city folks have become so accustomed as to consider it the proper and natural condition, that a very large proportion of the summer diarrhœa and the resulting mortality among the children in our cities is due. If any one wants ocular proof of the grosser forms of contamination to which city milk is exposed, he has but to visit any of the larger milk depots of the city, and to look at the nature and the quantity of the foreign material left upon the cloth strainers connected with the centifical machines used for separating the cream from the surplus milk of the day. If he does not then learn by seeing with his own eyes what he could otherwise hardly be brought to believe to be

possible, I am very much mistaken as to what are the real facts. Very fortunate indeed would it be, if only the firm and already dissolved impurities could also be thus readily separated out from the milk.

We, as physicians, have learned to know that milk about upon the point of changing will more disturb the digestion of a child than that which has already turned sour. I believe that the difference in the coagulation of breast and cow's milk in a child's stomach is more owing to the difference in the chemical reaction of these two milks, and in the amount of albuminoids contained therein, rather than to any difference in the nature of these albuminoids, for cow's milk, even when fresh, is of an acid reaction, while breast milk is neutral, or may be slightly alkaline. Then, too, cow's milk contains a much larger amount of albuminoids. Making cow's milk of a neutral or slightly alkaline reaction will render this curd finer and softer, and this is also accomplished by diluting with fluids or with soft solids, for which barley flour or malted extract serves well.

When it is desired to add cream to the diluted and sweetened milk when prepared for a child, I think it is better here in the city to use sweet cream raised by the centrifical machine, rather than that obtained by separation from standing, which necessitates it being just so much the older, when one of the chief troubles is that it is already too old. Such sweet cream is to be had of the large milk depots in the city.

I think that if physicians would instruct all their patients to demand that milk be delivered to them the same day that it is brought into the city from the country, this would become the more common custom of the trade, and that there would be a marked decrease in the infant mortality during the summer months, for if we cannot secure proper cleanliness in the care of the milk, we might at least thus shorten materially the time during which fermentative contaminations would have a chance to work out their harmful changes.

DR. GEORGE W. GALVIN exhibited an

IMPROVED INHALER,

which he described as follows: The inhaler is the invention of Dr. Geo. A. Evans, of Brooklyn, New York, and a full description of it, with a group of cases treated by him, was published in the *New York Medical Journal*, March 6, 1886. The great obstacle to overcome in the local treatment of pulmonary diseases, is the condensation of the vaporized medicament in the mouth, pharynx, and upper air passages. This has been overcome by Dr. Evans's instrument, which can reduce any fluid medicine or solution to so fine a degree that it can pass the bronchial tubes without condensing and reach the air cell of the lungs in which is stored the residual air. The changes in the lungs in pulmonary phthisis bring consolidation, tissue necrosis, putrefaction, decomposition, and septic absorption (I am not now considering tubercle, its nature, etc., but what is considered pulmonary consumption); it is natural to suppose that if we can convey to the lung an antiseptic vapor which will reach the part undergoing the pathological changes, we should obtain practical results from the local treatment of pulmonary affections.

The apparatus consists of an air condensor, an air chamber (flask) for preserving a continuous current of air, a three-mouthed bottle, "Wolf's;" a spray chamber having three mouths; spray tubes; respiratory tube; mouth-piece.

A spray of the solution in the bottle is thrown (by means of compressed air), against the opposite wall with sufficient force to produce a thorough churning with the atmospheric air, a fine vapor, resembling cigar smoke, which escapes through one of the openings in the glass air chamber to the respiratory tube.

During inspiration through the mouth-piece, air enters the middle mouth of the bottle and displaces the vapor of which the bottle is full. During expiration the air in its turn is displaced, passing out of the middle mouth of the bottle, to make room for the vapor which passes from the glass air chamber to the bottle, where it accumulates for the next inhalation.

A vapor produced by means of this apparatus condenses slowly, fifty cu. in. of a vaporized solution, containing carbolic acid, borax, glycerine and water, requiring twenty minutes of a temperature of 60° F. to entirely condense.

Ten cases are now reported in this journal with a cessation of symptoms which leads the patients to consider themselves cured. One case of my own far advanced in the second stage of chronic catarrhal pneumonia, after ten inhalations has gained in ten days two and one-half pounds, and to all intents and purposes is making a rapid recovery.

DR. W. EVERETT SMITH then exhibited a

SEMPLE'S ATOMIZING INHALER

which he has been using in connection with Williams' Pneumatic Cabinet. This inhaler was devised by Wm. F. Semple, of Mount Vernon, Ohio, in August, 1881, and consists of a glass receptacle containing an ordinary atomizer. The medicament is held in the bottom of the jar and is atomized either by a rubber hand bulb or more effectually by a cylinder of compressed air. The spray is projected against the top of the jar, the larger particles are condensed to be re-atomized and only the very finest portions of the spray escape through the inhaling tube.

The vapor thus inhaled has been so churned as it were and broken up that it is as fine and impalpable as smoke, and indeed resembles it in appearance. The mouth-piece to the inhaling tube can readily be removed and attached to a flexible rubber tube so that the vapor can be conducted wherever and in whatever direction one pleases. Any fluid that is readily atomized may be employed, the best menstruum being either fluid petrolatum or a 20 per cent. solution of glycerine in water. The resulting vapor will resist condensation for from five to fifteen minutes.

The atomizer was shown to be identical in its principles of construction with the Evans' Inhaler that was exhibited by Dr. Galvin, and when attached to the cylinder of compressed air produced as fine and perfect a vapor as Evans' apparatus. It has, however, these differences in its favor—is simpler in its construction, more easily cleansed and was perfected and in use *four years* before Dr. Evans set up his claim; yet Dr. Evans calls his atomizer a "*new instrument.*"

Further than this, Dr. Evans in his article in the *New York Medical Journal* of March 6, 1886, makes no mention of and gives no credit either to the Semple Inhaler or to the Globe Inhaler of Beseer.

Dr. Evans' apparatus is faulty in two particulars; the inhaling tube is long and is complicated with three glass globes which must be kept thoroughly antiseptic if we are to assign any dangerous activity to the numerous germs of disease. The patient under treatment must moreover inhale and likewise exhale through this one tube, the three glass globes and the Wolff bottle which contains the medication; each patient ought therefore to have an entire atomizer reserved for his own particular use, since it would be practically impossible to cleanse *thoroughly* all this complicated arrangement after each application.

Dr. Evans claims, however, as an advantage in favor of his apparatus that the vapor will resist condensation for twenty minutes. But is easily demonstrated that with any form of atomizer constructed upon these principles, the increase in the resistance to condensation depends *directly* upon the increase in the specific gravity of the fluid atomized and not upon the particular *shape* of the atomizer.

The very fact, however, that the vapor refuses to condense for so long a period as twenty minutes is an objection rather than an advantage to the method of treatment. Everyone grants that ordinary atomizers are of little benefit in the treatment of lung diseases, because the vapor from them condenses almost entirely upon the mouth and pharynx. At the same time no physician would demand that a patient suffering from pulmonary phthisis should actively inhale a medicated vapor longer than twenty or twenty-five minutes at any one time because of the physical exhaustion that would follow a longer treatment. If then the vapor that is inhaled resists condensation for so long a period as twenty minutes, Dr. Smith claimed that it is *just beginning* to be deposited upon the bronchial tubes when the treatment is stopped. He had been better satisfied with the results obtained by a vapor that did not resist condensation for nearly so long a period as that claimed by Dr. Evans.

At the same time Dr Smith claimed it to be a fallacy that by the use of either the Semple or the Evans' Inhaler atomized vapor can be deposited upon *lung cells*, especially if they are diseased so that they are unable to be readily expanded. The vapor when finally churned in these inhalers may be deposited and very likely is deposited upon the larger bronchial tubes, but we must remember that bronchial tubes are very different from air vesicles and that the volume of air which we ordinarily breathe—the *tidal* volume—reaches *directly* only the trachea and large bronchi, but enters the air cells in obedience simply to the law of the diffusion of gases.

Unless, therefore, some of the external pressure of the atmosphere be removed from the chest walls, as it is in the Williams' Cabinet, so that the *complemental* volume of air can be inhaled *involuntarily* into even remote air vesicles, the only medication that can effectually reach any decided area of lung cells must be gaseous in form. But however finely you may divide aqueous vapor under ordinary conditions of temperature and atmospheric pressure, you cannot make it gaseous, for a gas is not condensed except under enormous pressure and at exceedingly low temperatures.

Dr. Smith accordingly, while praising the inhalers that were exhibited, maintained that to derive their greatest benefit they should be used in conjunction with some form of the Pneumatic Cabinet. He has used the inhaler with the cabinet since last December, and has found the results to be highly satisfactory and encouraging.

Dr. Bowditch said that one of the important objects of any inhaler was to force the substance to be inhaled into the pulmonary structures as much as possible. Whatever the arrangement for inhalation may be, the object to be attained is the pulverization of the substance, and its forcible propulsion into the lungs. The inhaler shown by Dr. Smith is always employed in conjunction with the Pneumatic Cabinet, as an aid to its introduction or propulsion into the deeper parts of the pulmonary recesses.

Dr. Knight stated that the instrument exhibited by Dr Galvin was capable of producing a finely pulverized fluid, and he had never seen anything so fully answering this demand as the one shown at this meeting.

MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA.

Stated Meeting, May 19, 1886.

Vice-President, Wm. H. Taylor, M.D., in the Chair.

T. E. McArdle, M.D., Secretary.

Dr. Bermann presented a bottle full of

POLYPI REMOVED FROM A PATIENT'S NOSE,

where they had been growing for the past seven or eight years. The turbinated bones were atrophied and the septum driven to one side. Some of the tumors were gelatinous, but most of them were of a fibroid character.

Dr. Jos. T. Howard read a paper on

UROGLAUCIN.

About 1845 Heller discovered in urine a peculiar substance which he called uroxanthine, which, by further treatment with acid reagents at a high temperature, yielded a blue uroglaucin and a red pigment, urrhodine. These coloring matters, he proved, were similar to those which had been previously discovered by Braeonnot, in the urine, composing a substance which he (Braeonnot) called cyano urine, and Virchow and Kleitzinsky showed the identity of urrhodine with indigo red, and uroglaucin with indigo blue. It is the latter, apparently, occurring in the urine of a patient under my care, that it is proposed to speak of now. I say apparently, because the sequel may prove it to be something else; consequently, before detailing the case it may be of interest, if not edifying, to briefly consider the history and sources of "indigo blue," with its relation to the urine generally.

Indigo blue was first mentioned by Dioscorides as

indikon, next by Pliny as indicum. When first introduced into England it was called "indico." The principal source of this familiar coloring matter is, as you well know, the numerous indigiferous plants, mainly the indigifera dinctoria, anil, satisneurium and polyganum. In the process of manufacturing indigo the plants are steeped in water for a few moments, and after their removal from the liquid the latter is exposed to the air and frequently agitated to facilitate the absorption of oxygen, which produces the blue color in the water. If the water be allowed to rest awhile indigo is deposited as a pigment insoluble in water, alcohol, oils, dilute alkalies, and hydrochloric acid, but if slowly heated it volatilizes in purple vapor, which afterwards becomes condensed in delicate crystals, the chemical composition of which is expressed by the formula, $C_{16}H_5NO_2$.

In the urine, as stated by Heller, indigo was found in combination with urrhodine, forming the compound uroxanthine; from which his process of extracting uroglaucin was somewhat as follows: Urine recently voided is mixed with a hot solution of acetate of lead filtered and freed from an excess of the acetate by sulphuretted hydrogen, and boiled to free it from excess of the latter. The hot fluid is now constantly stirred and gradually added to an equal volume of concentrated fuming hydrochloric acid, which, if uroglaucin be present, should cause it to assume a greenish and very dark blue color. If the mixture assume a violet or red tint only, no indigo is present. If it does become blue, as just stated, and is allowed to stand for some time, a copper-red, shining, metal-like, dark crystalline pellicle appears upon the surface. After twelve hours the fluid is diluted with an equal portion of cold water, well agitated, and put aside for twenty-four hours, when a heavy deposit is formed which is separated by filtration and washed with boiling water until the washings have a neutral reaction; it is then washed with dilute spirits of wine and dried over sulphuric acid in the water stove. The dry filter is now washed with pure ether as long as any red color, urrhodine, can be obtained. For this purpose large quantities of ether, and a proportionate amount of time, are required to get rid of the urrhodine. Next, all that portion of the filtering paper which shows no blue deposit is cut off, and the remainder, divided into small pieces, put into a balloon and boiled strongly with absolute alcohol. As often as the alcohol assumes a sky blue, or greenish, color, it is poured off and a new portion added until, by protracted boiling, it becomes entirely colorless. The alcoholic extract, which has an acid reaction, is filtered whilst boiling hot, then evaporated down to one-half, and put aside in a well-stoppered bottle. When the fluid cools uroglaucin is deposited in spider-like petaloid.

The first record of blue coloring matter occurring in the urine is attributed to James Planeus, 1767; the first chemical description of a blue or violet coloring matter being found in the same was given by Braeonnot, who, as before mentioned, called it cyanouriне. In one of Braeonnot's cases the urine, voided with difficulty, was of an intense yellow, which after standing gave a blue deposit. He relates another case reported by M. Julia, in which the blue color was attributed to the presence of prussiate of iron. (See *Ann. de Chem. de Phys.*, xxix, p. 252.) Braeonnot was followed in his discovery by Heller, Prout, and others.

Now in no instance on record, so far as I have been able to ascertain, except, perhaps, the cases of James Planeus and of M. Julia, can it be inferred that indigo was ever found spontaneously dissolved in the urine, but when present in that fluid was discovered only after a long and tedious chemical process such as has been detailed above; nor is there any account given of a simple blue urine having been emitted; consequently the case that I now present is the third, if not the first, to be chronicled in which urine of a dark blue color has been voided. For such indeed it was at that time, and did not become blue from decomposition after standing, as was the case mentioned of Braeonnot, and which it might have done in that of M. Julia, either within the bladder or after emission.

It is hardly probable, in the case now presented, that the change occurred whilst the urine was within the bladder, because the patient had, but a very short time before voiding the blue, passed a clear-colored urine; evidently, then, it was not in that vessel sufficiently long to undergo fermentation, although indican possibly may have been present in the urine previously passed without attracting attention, for Schunek says: "Urine containing uroxanthine exhibits no remarkable or peculiar appearance whatever to the naked eye; it is acid clear and of the usual color." This learned investigator found indican so often in urine that he concluded it must be a normal constituent of the fluid. Heller, however, found it most frequently in persons with disease of the kidneys and spinal cord—a circumstance which, as regards the last mentioned, coincides more nearly with the pathological condition of my patient, who for some time had been laboring under a severe attack of sciatica, in which the medulla spinalis, or its appendages, were more or less involved.

The following is the case: W. R., æt. 63, tall, of a spare frame, and sanguino-melancholic temperament, general health good, complained of great pain in the back and down the left lower extremity along the course of the great sciatic nerve and its branches, which, after a regimen of counter irritants, purgatives, and alteratives, showed signs of abatement and gave promise to the patient of an early recovery. One day after the usual morning visit, at which time all things seemed to have been progressing favorably, I was sent for in great haste, and upon entering the chamber of the man found him and his family in great consternation and, perhaps, some little alarm, on account of the blue urine exhibited, which the patient had but a short time before passed. Observing the anxiety of his distressed wife, and others around him, the invalid affected to treat the matter lightly, and jocosely remarked that he "had often heard of 'blue bloods,' but was not before aware of his being one of them." Although unable at the time to explain the phenomenon, I was satisfied from the patient's general condition that there was no cause for

uneasiness regarding him, and so expressed myself. Upon examining the urine I found it to be of a deep indigo-blue color, and being assured that it had been voided in that condition I was satisfied. If proof had been necessary it was present in stains upon the bed linen.

I very much regret that a portion of this urine was not taken for chemical investigation. How indigo is obtained from indigiferous plants, uroxanthine, and the urine itself, has already been explained; perhaps, by reverting to that part of the subject we may be obtain at least a negative clue to its formation in the case before us. The improbability, in my case of the blue color having resulted from decomposition of the urine within the bladder, or after it passed into the bidet, has been considered, and we know it was not subjected to any chemical process similar to that before detailed. Then, from whence came it? Perhaps it originated in the alimentary canal and was taken up by the absorbents through which it found its way into the blood, causing the patient to be, as he indeed supposed himself, blue blooded; from the blood it was eliminated by the kidneys, passed into the bladder, and from thence into the vessel. This is a very rational conclusion, when we consider the fact that the patient had been for some time taking iodide of potash, and having taken a meal that morning composed principally of amyllaceous substances, it may be that some of the iodide remaining in the stomach was decomposed by the gastric juice setting free the iodide, which, coming in contact with the starch in solution struck the blue color which was imparted to the blood in the manner indicated. This deduction is sustained by the well-known fact that one millionth part of iodine will give the blue color to a solution of starch outside of the body; from which it may be reasonably inferred that, in the instance in question, the same reaction occurred within the body; but if this be the case, was it uroglaucin or simply a blue color?

Dr. Acker had seen one such case in an hysterical woman who was suffering from chronic Bright's disease. She passed a pint of urine of a decidedly blue color. Uroglaucin has been discovered when disease of the alimentary tract is present; also in granular disease of the kidney.

Dr. W. W. Johnston presented a specimen of

PERITONITIS IN TYPHOID FEVER, WITHOUT PERFORATION.

Dr. A. Y. P. Garnett disagreed with Dr. Johnston as to the diagnosis. The only specific evidence present was slight enlargement of Peyer's patches. Was there not some tubercular degeneration of the mesenteric glands? Peritonitis may occur in many ways and from many causes.

Dr. Johnston contended that the characteristic lesions of typhoid fever were present. There are no determining symptoms except a morning decline and an evening exacerbation, together with the duration of the fever. Of course all local inflammations must be excluded. In the case under consideration the fever had lasted two weeks, with a morning decline and an evening rise of temperature. At the autopsy he found enlarged and inflamed Peyer's patches and solitary glands. There was also acute enlargement of the mesentary glands.

Dr. Taylor remarked that John Harley declares that in the Delta of the Mississippi some cases of malarial fever are found which cannot be distinguished from typhoid fever, because ulceration of Peyer's patches is present.

Dr. Johnston said he would include such cases under the head of typhoid fever.

Dr. D. S. Lamb presented a specimen of

PYO-SALPINX.

Uterus and appendages covered generally with lymph. Right Fallopian tube, distal portion, dilated and filled with pus, which readily escaped from fimbriated end, on pressure, into peritoneum. Ovaries showed small aqueous cysts.

From Mrs. C., aged about 35. A few hours after a fright on April 27, she had acute pain in the abdomen, followed by general peritonitis, and death one week afterwards. The necroscopy showed the intestines distended with flatus, all the abdominal viscera more or less united by adhesions, and a large quantity of pus in the cavity. The explanation of the case seems to be that the pus from the Fallopian tube found its way, at the time of the fright, into the peritoneal cavity and set up general inflammation, from which she died.

Dr. Lamb also presented a

LARYNX AND PORTION OF TRACHEA OF GENERAL TUBERCULOSIS.

They were taken from a man aged about 30, who had general tuberculosis. The mucous membrane shows many tubercles of the size of small pin heads, projecting from the surface; large ragged ulcer on under surface of base of epiglottis; another on each vocal cord, which was nearly destroyed; one on left cricoid, and several in trachea.

Necroscopy.—Much emaciation; dura mater normal; arachnoid showed numerous opacities, especially over sulci of convexity; Pacchionian bodies abundant along longitudinal sinus; pia mater along each middle cerebral artery showed minute tubercles, thick lymph and adhesions; there was lymph also on upper surface of left cerebellar hemisphere near median fissure. Brain substance generally consistent and normal, but fornix softened to a pulp; softening also on under surface of right optic thalamus; ventricles much dilated and full of turbid liquid; right choroid plexus indurated; small cavity in pineal gland. Lungs showed old adhesions, especially at apex of right: filled with miliary tubercles; large vomicæ in upper lobes. Liver normal. Spleen somewhat lobulated, firm old adhesions. Pancreas normal. Stomach not opened. Small intestine; lower portion showed ulcers, mainly occupying Peyer's patches, varying in size, and having the long diameter parallel with intestinal axis; corresponding peritoneum covered with minute tubercles and bands of adhesion. Vermiform appendix showed hour-glass contraction about midway of the tract; mucous coat of that portion opening into cæcum was deeply ulcerated; terminal portion nearly closed by the con-

traction named and filled with pus; its mucous membrane entirely destroyed by ulceration. Cæcum showed ulceration. Mesenteric and gastro-hepatic glands enlarged. Suprarenal capsules showed small tubercles (?). Kidneys normal. Bladder distended.

MISCELLANEOUS.

INTERNATIONAL CONGRESS.—The subject of Public and International Hygiene is likely to occupy an important place in the discussions of the ninth International Medical Congress, or rather of its fourteenth Section. We are glad to see that Mr. Simon is anxious to attend the Congress and take Vice-Presidentship of the Section. Mr. Simon, indeed, speaks in his letter responding to the invitation with some doubt as to his power to attend, but with a clear intimation of his desire to do so. Dr. B. W. Richardson and Dr. Thudichum both reply in most cordial terms to similar invitations, and accept a similar office. To those who know the imperfect health which Mr. Simon has lately enjoyed, the significance of his conditional acceptance is very great. The promoters of the Congress will take this as a sign of British good will towards their great and responsible undertaking. The important subject of International Quarantine is to be raised by the president of the section, Joseph Jones, M.D., of New Orleans. We could send to the discussion of such a subject no greater authority than Mr. John Simon. We are glad to be able to report a very general desire among leaders in London to be present at Washington in 1887. Whatever hesitation they felt some time ago in view of the divided state of feeling in America is now steadily giving place to a desire for the success of the Congress, and a wish to do anything they can to further it. We feel quite justified in saying that our best men in England and Scotland are preparing to go if they get any indication that their presence will be acceptable. Sir Andrew Clark, Sir Spencer Wells, Professor John Chiene, Professor Fraser, and we believe Sir William Turner, are all, according to our information likely to go.

It is not America alone that is interested in the success of the meeting at Washington, but the profession throughout the whole world, and we might add the world itself. When our profession meets internationally it is of good omen. We not only stimulate fraternity and scientific rivalry amongst ourselves, but every thought in advance and every medical discovery is a great boon for the human race and for all nations. We urge on members of our profession in the empire to strain a point to be at Washington on or before September, 1887, where, if report is to be trusted, a very hospitable reception awaits them.—*Lancet*, August 7, 1886.

THE HYGIENE OF THEATRES.—Given a spacious theatre, crowded during an evening performance for a period of hours, what is the difference of the effects on its atmosphere produced by the electric light or by gas, particularly as to the temperature and the generation of carbonic acid? Such is the problem which Dr. P. Renk, Professor of Hygiene at Munich, has just set himself to solve. Taking the Theatre Royal of that city, which is lighted by 1400 lamps on the Edison system, he tested the two kinds of illuminants, with the following results: During a representation which lasted from 5:30 P.M. to 10:15 P.M., and was witnessed by 1790 people, he found that while the rise in temperature in the pit was 11.1° C. with gas, it was only 7.7° C. with the electric light, and that in the upper gallery it was 10.7° with the former, and not more than 7.4° with the latter. As to the increase in the production of carbonic acid, the results were equally important. In the pit it was 2176 with gas to 1221 with the electric light, and in the upper gallery as 2855 with the first to 1430 only with the second.—*Medical News*, July 24, 1886.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM AUGUST 21, 1886, TO AUGUST 27, 1886.

Major A. A. Woodhull, Surgeon, granted leave of absence for fourteen days, to take effect on or about Sept. 1, 1886. (S. O. 197, A. G. O., Aug. 25, 1886.)

Major M. K. Taylor, Surgeon, leave extended one mont (S. O. 195, A. G. O., Aug. 23, 1886.)

Capt. J. V. Lauderdale, Asst. Surgeon, granted leave of absence for two months, with permission to apply for one month's extension, when his services can be spared. (S. O. 195, A. G. O., Aug. 23, 1886.)

First Lieut. Chas. M. Gandy, Asst. Surgeon, on expiration of his present leave of absence, relieved from duty in the Dept. of the East, and assigned to duty in the Dept. of Texas. (S. O. 195, A. G. O., Aug. 23, 1886.)

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY, DURING THE WEEK ENDING AUGUST 28, 1886.

Clark, John H., Medical Inspector, ordered to special duty, Portsmouth, N. H., and Widow's Island.

Kindleberger, D., Medical Inspector, ordered to hospital, Washington, D. C., Oct. 1, 1886.

Gihon, A. L., Medical Director, ordered to hospital, Mare Island, Cal., Oct. 15, 1886.

Robinson, Somerset, Medical Inspector, detached from hospital, Mare Island, Cal., Oct. 15, 1886, and wait orders.

Spear, J. C., Medical Inspector, ordered to Naval Laboratory, New York, Sept. 28, 1886.

Bloodgood, Delevan, Medical Director, ordered to hospital, Norfolk, Va., Sept. 29, 1886.

Taylor, J. Y., Medical Director, ordered to Naval Laboratory, New York, Sept, 29, 1886.

Dean, R. C., Medical Director, detached from Naval Hospital, New York, and wait orders.

Simon, W. J., Surgeon, detached from U. S. S. "Constellation," and special duty at Naval Academy, Annapolis, Md.

Henry, C. P., Asst. Surgeon, ordered to hospital, Philadelphia, Pa.

Pitts, H. B., P. A. Surgeon, detached from hospital, Philadelphia, Pa., and wait orders.

Stone, E. P., Asst. Surgeon, ordered to receiving ship "New Hampshire."

CORRIGENDUM.

In Transactions of the Chicago Gynæcological Society, June 18th—appearing in the last issue of THE JOURNAL—Dr. Wm. H. Byford is reported to have read a paper entitled "A Study of the Cause and Treatment of Pelvic Hæmatoceles." Dr. H. T. Byford was the author of the paper.